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The Price Elasticity of Demand for Pharmaceuticals amongst High Income Older People in Australia: A Natural Experiment

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Abstract

This paper estimates the price elasticity of demand for pharmaceuticals amongst high-income older people in Australia. It exploits a natural experiment by which some people gained entitlement to a price reduction through the Commonwealth Seniors Health Card (CSHC). To the author's knowledge, this is the first study of the price elasticity of demand for pharmaceuticals amongst older people that draws on a natural experiment with a control group. The preferred model is a nonlinear Instrumental Variable (IV) difference-in-difference regression, estimated on nationally representative repeated cross sectional survey data using the Generalised Method of Moments. No significant evidence is found for endogenous card take-up, and so cross-sectional estimates are also considered. Taking all of the results and possible sources of bias into account, the 'headline' estimate is -0.1, implying that quantity demanded is not highly responsive to price.

JEL codes: D12; H42; I11
Keywords: price elasticity; pharmaceuticals; Australia

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1. Introduction

Expenditure on pharmaceuticals in Australia and in many other countries has increased considerably in recent years. Between 1995 and 2004, it increased relative to GDP in 21 of 24 OECD countries for which data are available. The United States had the largest relative increase, from 1.2% to 1.9% of GDP (OECD, 2007: 8-9). In Australia, total pharmaceutical expenditure was A$10.9 bn in 2004-05 ($536 per person), having increased annually by 8.9% in real terms over the previous decade (Australian Institute of Health and Welfare, 2006: 105; 64). These increases have prompted concerns over sustainability and cost containment of pharmaceutical insurance, particularly public insurance such as Australia's Pharmaceutical Benefits Scheme (PBS). The price elasticity of consumer demand determines how total expenditure responds to price. It is also a key parameter in assessing the optimal level of co-payments, given the trade-off between the utility gain of risk-pooling and the loss associated with moral hazard (Arrow, 1963; Pauly, 1968; Manning & Marquis, 1996).

Older people account for a disproportionate share of pharmaceutical consumption. In Australia, government PBS expenditure per capita on those aged 65 and over is 7 times higher than on those aged under 65 (calculated from Australian Government, 2007b: Table C2). Continuing structural ageing of the population will ensure that their share of pharmaceutical expenditure will continue to grow. In Australia, it is expected that people will be increasingly affluent in retirement (Australian Government, 2007b: Chart C6). The aim of this paper is to present estimates of the price elasticity of demand for pharmaceuticals amongst high-income older people in Australia. I exploit a change in the income eligibility threshold for the Commonwealth Seniors Health Card (CSHC). Holders of CSHC's, or another concession card, pay a much lower contribution than general patients. This 'natural experiment' is assumed to represent an exogenous price change for those who take-up the card.

The remainder of this paper is organised as follows. Section 2 reviews previous studies of the price elasticity of demand for pharmaceuticals. This is followed by a description of the CSHC natural experiment in Section 3. I describe the methods, data and present descriptive statistics in Section 4. The effect of the CSHC on price is discussed in Section 5. The final two sections present results and a conclusion. Results of alternate specifications are contained in an Appendix.

2. Previous studies

The RAND health insurance experiment remains the most significant work on demand for health care (Newhouse & The Health Insurance Experiment Group, 1993). Using these data, the price elasticity of demand was estimated to be -0.17 to -0.22 for health care overall and perhaps slightly larger for outpatient care (between –0.17 and –0.31) (Manning *et al.*, 1987). To the author's knowledge, the price elasticity for drugs was never calculated from the RAND data. These cannot be derived from published results because of the impact of the maximum dollar expenditure included in the plans. However, the predicted demand response for drugs (Leibowitz *et al.*, 1985: Table 4) is very similar to that of outpatient care (Newhouse & The Health Insurance Experiment Group, 1993: Table 3.2). Indeed Leibowitz et al. (1985) found no statistically significant difference for all but one of the sites considered. Whilst this result can be regarded as a benchmark, there are limitations to what can be drawn from the results. The RAND experiment was conducted around thirty years ago and it did not include older people in the sample. Institutional differences between countries may also play a role.

Rice & Matsuoka (2004) consider whether price elasticity amongst seniors may be different to that of younger people. Determinants include necessity, availability of substitutes and the proportion of income spent on the product. Seniors are more likely to have chronic ailments, for which drugs are necessary and for which there is little substitute treatment. These two factors would lead them to be less responsive to price. On the other hand, they generally spend a greater proportion of their income on pharmaceuticals, which would have the opposite effect. This is less likely, however, for a high-income population. It is therefore hypothesised that the population of high-income older people are less responsive to price than younger or poorer groups.

There are few studies of responses to price in demand for pharmaceuticals for Australia and all rely on time series methods. The most relevant is by McManus et al. (1996) who estimated the effect of the introduction and increase in co-payments in 1990 and 1992 on the pharmaceutical consumption of community patients and a group of elderly returned servicemen and women. Whilst they do not provide an elasticity estimate, this can be calculated from their results for the group of returned service people. For this group, the introduction of a $2.50 co-payment was associated with a decrease in consumption by

around 20% for both essential and discretionary drugs, implying an arc elasticity of around –0.11. This calculation ignores the income effect associated with the concurrent introduction of a pharmaceutical allowance. But this allowance was only $135 per year (equal to 1.7% of the service pension) and so the resulting bias would be small if not negligible. Exploiting increases to the co-payment for general patients in 1986, Johnston (1991) estimated a much larger price elasticity of –0.47 for high price pharmaceuticals and –0.24 for safety net (high user) patients. Relying on earlier, smaller co-payment increases for general patients Harvey (1984) estimated an elasticity of between –0.1 and –0.15.

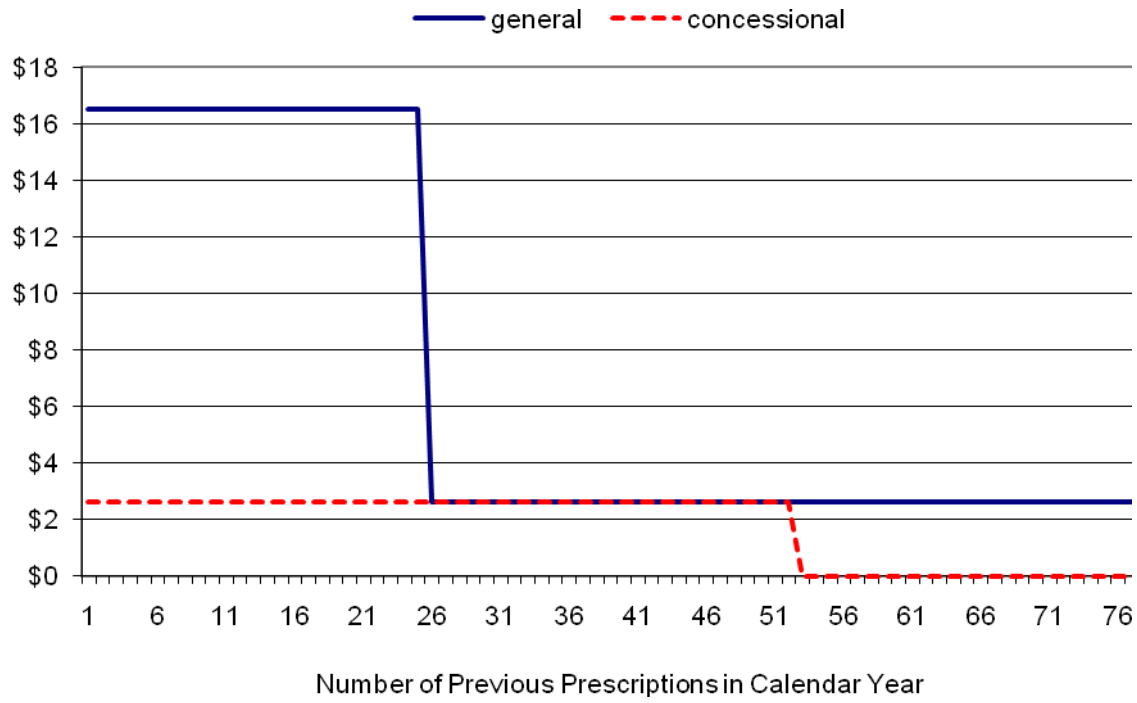
Rice & Matsuoka (2004) reviewed North American studies on the impact of cost-sharing on the health and health service utilisation amongst seniors aged 65 and over. They identified sixteen studies that analysed prescription drug use, most of which found evidence of a decline in utilisation associated with cost-sharing. Five of these exploited natural experiments, three of which related to the same policy change in Quebec. None of the studies reviewd by Rice & Matsuoka (2004) reported estimates of price elasticity. More recently, Contoyannis et al. (2005) estimated price elasticities using the same Quebec natural experiment, which were between -0.12 and -0.16 for the group of people aged 65 and over. However, their study was limited by the absence of a comparison group. It relied on time trend terms to capture differences over time. An example of an effect that could not be accounted for is the introduction of income-contingent Pharmacare premiums, which coincided with the price change. These may have influenced consumption through an income effect. The study also estimated elasticities for sub-populations stratified by income. For the low-income population, the elasticity was close to zero (an average of +0.04 across expenditure deciles). In contrast, it was large (an average of -0.69) for the high-income population, with the middle-income population roughly midway. The subject of the present study is a high income population.

3. The Commonwealth Seniors Health Card Natural Experiment

The Pharmaceutical Benefits Scheme (PBS) is Australia's universal system of pharmaceutical insurance. For listed medications, co-payments are capped at a fixed amount (the co-payment), which does not vary with the full cost of the medicine.[2] The PBS is a two-tiered system, with 'general' and 'concession' components. Concession patients include pensioners, the unemployed and low-income earners. Since 1990, all concession cardholders have been required to make a small co-payment, but it has remained much smaller than the general contribution. Both classes of patients are also covered by a 'safety net'. Concessional patients who reach the expenditure threshold in a given year are not required to make co-payments on additional purchases. General patients who reach their threshold pay the concessional price for additional purchases. By way of illustration, the resulting (non-linear) price schedule is shown for general and concessional patients for 1995 (Figure 1).

[2] Two qualifications are necessary. A patient may be required to pay a Brand Premium or Therapeutic Group Premium if they choose more expensive medications instead of cheaper medications that are judged to be of equal medicinal value. Secondly, where the medication's full cost is lower than the notional co-payment, the patient pays the full cost.

Figure 1 Indicative Price Schedule for PBS Pharmaceuticals for General and Concessional Patients, 1995 (AU $ per prescription)*



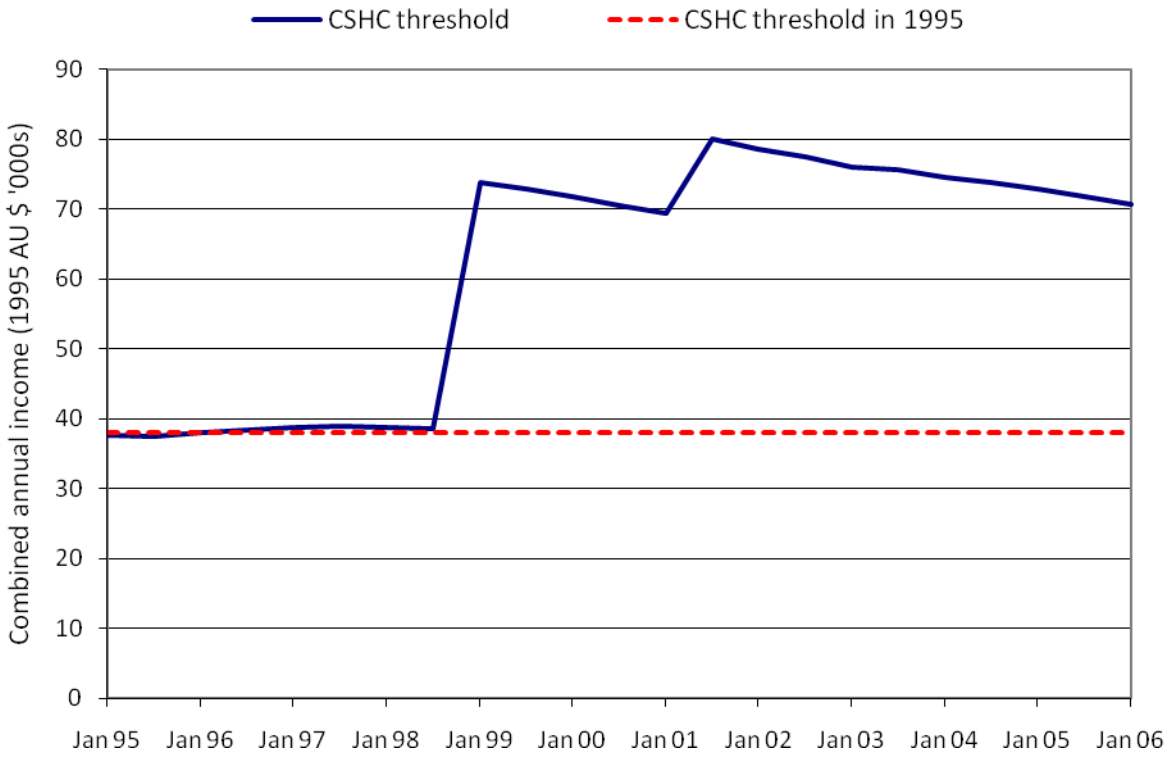
Source: Australian Government Department of Health and Ageing (2007)

* This schedule is illustrative, since some listed pharmaceuticals are priced below the level of the general co-payment. The price also excludes any Brand Premium (the Therapeutic Group Premium is not relevant as it was not introduced until 1998). These issues are discussed further in Section 5. The horizontal axis is also indicative, since the safety net is invoked after expenditure crosses a threshold, rather than the actual number of prescriptions. For this figure, it is assumed that all prescriptions are purchased at the full co-payment price.

The Commonwealth Seniors Health Card (CSHC) was introduced in July 1994. Its original purpose was to provide a concession to people of age pension age who met the income eligibility test, but who did not receive an age pension for other reasons. The majority of such people were excluded from the age pension due to its assets test or residency requirements. The reform of analytical interest was a large increase in the CSHC income eligibility threshold. In January 1999, the threshold was almost doubled to $40,000 per annum for singles and $67,000 for couples. Income was defined as the previous year's taxable income, adjusted for rental property losses, foreign income and fringe benefits. It was not indexed to inflation. It was increased nominally in July 2000 and more substantially in July 2001, to $50,000 for singles and $80,000 for couples (Australian Government, 2007a: Section 4.10.7.50). A time series of this threshold for couples is shown in Figure 2, and the

corresponding pattern for singles has a similar pattern. The real value of the 1995 threshold is also shown for all years. An income that falls between the two lines would entitle a coupled person to a concession card at that point in time, but not prior to 1999.

Figure 2 CSHC Income eligibility thresholds for couples (1995 AU $ '000s per annum)



Source: Australian Government (2007a)

4. Methods, Data and Descriptive Statistics[3]

Price elasticity of demand is defined as the percentage change in quantity demanded divided by the percentage change in price, other things equal. The formula used to estimate the arc elasticity associated with a discrete price change is the midpoint formula:

$$E = \frac{C_1 - C_0}{(C_1 + C_0)/2} \div \frac{P_1 - P_0}{(P_1 + P_0)/2} \qquad (1)$$

[3] Analysis was conducted using Stata V9.2 and SAS V9.

where C_1-C_0 is the estimated effect on consumption of a price change equal to P_1-P_0.

The approach taken in this paper is to separately estimate the effect of health care card possession on consumption, and the effect of a health care card on price. To reflect this, the subscript notation in (1) is changed to:

$$E = \frac{C_{H=1} - C_{H=0}}{(C_{H=1} + C_{H=0})/2} \div \frac{P_{H=1} - P_{H=0}}{(P_{H=1} + P_{H=0})/2} \qquad (1a)$$

where and $C_{H=1}$ and $C_{H=0}$ are the expected consumption with and without a health care card, and similarly for price, *P*.

All the methods described below estimate Marshallian (uncompensated) own price elasticities for pharmaceuticals. These elasticities include the pure price effect and the income effect of a price change. However, the compensated and uncompensated elasticities are almost identical in the present analysis, since the share of total income spent on the relevant good (PBS pharmaceuticals) is small. To illustrate, consider the Slutsky equation, expressed in terms of elasticities:

$$e_{x^M,P} = e_{x^H,P} - s_x e_{x^M,I}$$

Where x^M represents Marshallian demand and x^H represents Hicksian demand. This equation states that the Marshallian price elasticity of demand is equal to the Hicksian (compensated) price elasticity of demand minus the share of income spent on *x* multiplied by the income elasticity of demand. The methods in this paper directly estimate the term on the left hand side. The pure price elasticity is the first term on the right hand side. For the population studied here, the share of income spent on PBS pharmaceuticals is approximately 1%. Assume that the income elasticity is large (say 1.3 as estimated by Moran & Simon, 2006). The two price elasticities would differ by 0.01 * 1.3 = 0.013. The pure price elasticity will be slightly smaller (slightly less negative) than the uncompensated elasticity. Since the difference is of trivial magnitude, this issue will not be given further attention.

The rest of this section is devoted to the methods used to estimate the first term in equation (1a). However, the non-linear price schedules make the second term nontrivial as well. The estimated effect of the health care card on price is discussed in section 5.

4.1 Difference-in-Differences with Instrumental Variables

In the economic evaluation literature, the expected effect of a treatment on the treated (denoted *Gain*) is the difference in the expected outcome (*Y*) between treated individuals and their expected hypothetical outcome in the absence of treatment.

$$E(Gain) = E(Y^T \mid T = 1) - E(Y^T \mid T = 0) \quad (2)$$

where Y^T is the outcome for members of the treatment group and *T* denotes treatment. The second term in (2) is the 'missing counterfactual', since it is unobserved. With panel data or repeated cross sections, we observe the outcome at two points in time. *Gain* can be redefined as the expected impact on the change in outcomes over time:

$$E(Gain) = E(Y_1^T - Y_0^T \mid T = 1) - E(Y_1^T - Y_0^T \mid T = 0) \quad (3)$$

In the difference-in-difference model, a comparison group is selected which is not subjected to treatment. Change over time in the outcome for the comparison group is assumed to be equal to that of the treatment group in the absence of the intervention:

$$E(Y_1^C - Y_0^C \mid T = 0) = E(Y_1^T - Y_0^T \mid T = 0) \quad (4)$$

Where Y_t^C denotes the outcome at time *t* for members of the comparison group and Y_t^T denotes the outcome for members of the treatment group. Substituting (4) into (3) enables the treatment effect to be estimated as a function of observed outcomes:

$$E(Gain) = E(Y_1^T - Y_0^T \mid T = 1) - E(Y_1^C - Y_0^C \mid T = 0)$$

Since other observable characteristics (*X*) may change over time heterogeneously between groups, *E(Gain)* can be estimated in a linear regression model:

$$Y_{it} = \alpha + \phi t + \delta G_i + \gamma G_i t + \beta X_{it} + \varepsilon_{it}$$

where *G* = 1 if the individual is in the treatment group (before or after treatment) and zero otherwise, *t* = 0 in the pre-intervention observation and 1 in the post-intervention observation. The parameter γ represents E(*Gain*).

In the difference in difference model, treatment (T) is assumed to be assigned. In other applications, treatment is not assigned, but rather, eligibility is assigned through a policy

change. In the case of the CSHC, relatively high income elderly people (*G*=1) gained eligibility at *t*=1. Not all people who were eligible took-up the card. People self-select and so treatment may be endogenous. Some members of the treatment group also possessed a card prior to treatment. If, however, there is a strong correlation between *Gt* and health concession card status (*H*): P(*H*=1 | *Gt*=1,*X*) >= P(*H*=1 | *Gt*=0,*X*) and this policy change can be assumed to be exogenous, then the policy change can be used as an instrument for health care card status. In this approach, the estimated effect is the Local Average Treatment Effect for those who took up the card (Imbens & Angrist, 1994). Thus the linear difference-in-difference instrumental variable model is:

$$Y_{it} = b_0 + b_1 t + b_2 G_i + b_3 H + \beta X_{it} + \mu_{it} \qquad (5)$$

where $H = \alpha + \gamma G_i t + \phi t + \varphi G_i + \delta X_{it} + \nu_{it}$

and hence *Gt* is the instrument for *H*.

The key assumption in this linear model is that the time trend (in levels) is common to both groups. Alternative assumptions, such as a common trend in logs are also possible. I return to the issue of constant trends in levels or logs in section 4.3, by considering two post-intervention samples.

4.2 Data

The analysis was conducted on repeated cross-sectional National Health Surveys (NHS) of 1995, 2001 and 2004-05.[4] The NHS is a nationally representative survey conducted by the Australian Bureau of Statistics. The surveys were conducted face-to-face and the sample sizes were 52,838; 26,863 and 25,906 persons respectively (Australian Bureau of Statistics, 1996; 2003; 2006).

A treatment group was selected in each year. It consists of people aged 65 years and over whose 'real' income (inflation adjusted) would have qualified them for a CSHC after the policy change, but not in 1995. A first comparison group consists of people in the same age group, but with incomes that would qualify for a concession card in every year. A second comparison group consists of men aged 50-64 and women aged 50-59 whose incomes are in

[4] The 'Basic' Confidentialised Unit Record Files (CURFs) were used.

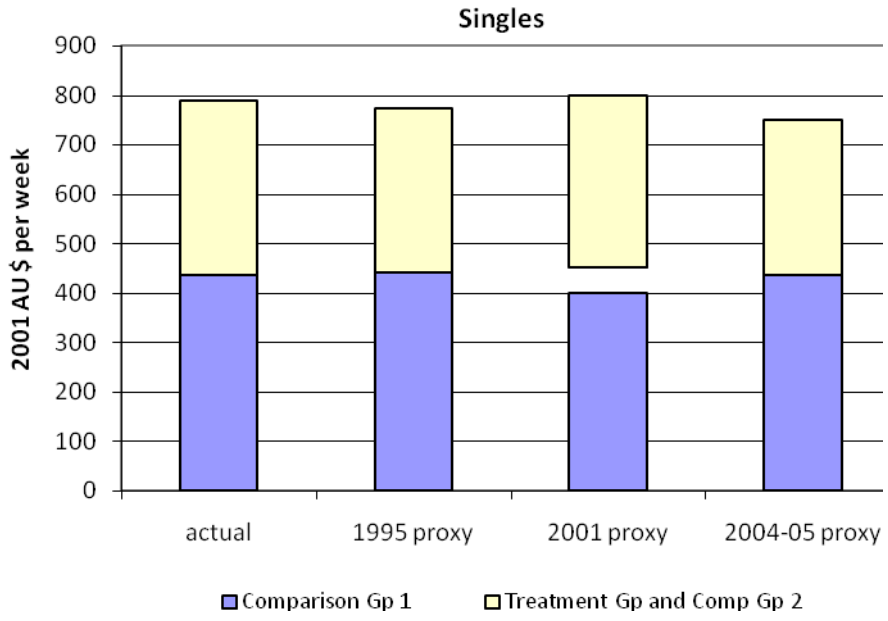
the same range as the treatment group.[5] A third comparison group, consisting of higher income older people who would not qualify them for a CSHC in any year was also considered, but abandoned due to small sample size. The groups were selected under the same criteria in each year rather than following particular cohorts over time. Thus I assume no cohort specific effects on drug consumption. It is noted that the groups are selected with error for several reasons. Income as recorded in each NHS corresponds to the ABS 'current' income measure. This is derived from the amounts received in the previous financial year from business and property and the current amount usually received from wages and salaries and other income (Australian Bureau of Statistics, 1996). In contrast, income for the purpose of CSHC eligibility is annual income in the previous year, or an estimate of annual income for the current year if a change in circumstances can be demonstrated (Australian Government, 2007b: Section 3.9.3.30). Each member of a couple reported their own income in 1995. In the other two years, one person reported their own and their spouse's income. In 2004-05, income of couples was not distinguishable from that of other household members. Thus couples who lived with anyone else were excluded from all years. Income is provided in ranges. However, by using all available income variables (including income ranges; income deciles and equivalised income deciles), it was possible to utilise what appear to be rather successful approximations to the income thresholds.[6] Figure 3 shows the ideal income range for selecting the treatment group amongst single and coupled respondents and the approximations that were used. The boxes with a diagonal fill pattern

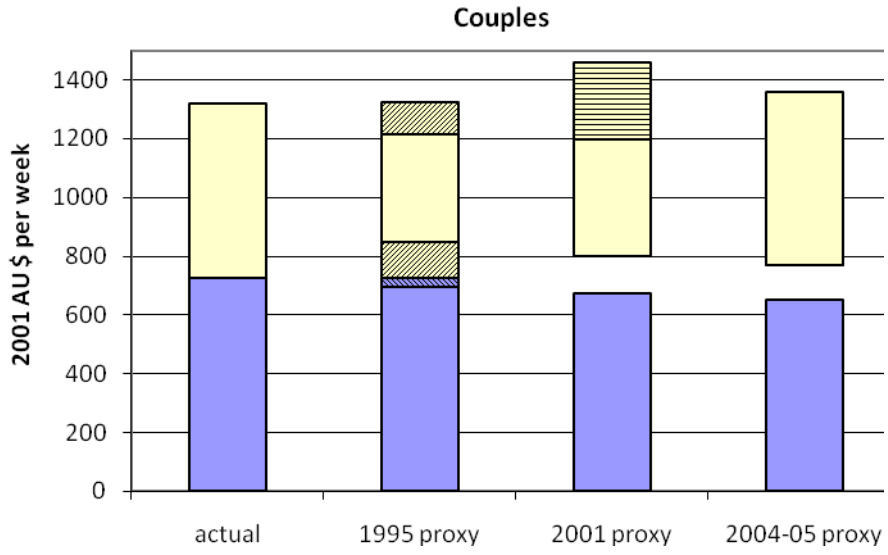
[5] The age eligibility threshold for women is gradually increasing from 60 to 65 years. The threshold was 60 years at the start of 1995 and it had increased to 62.5 by June 2005. Since age is provided in five-year categories on the files, women aged 60-64 have been excluded from all analysis.

[6] In NHS 1995, the groups were selected using a categorical personal income variable for the respondent (and their partner where applicable) and a variable for decile of equivalised income of income units. The equivalence scale used by ABS in the NHS 1995 file was a simplified Henderson scale. In NHS 2001, groups were selected using categorical variables for personal income, income unit income and decile of (modified OECD) equivalised income of income units. In NHS 2004-05, groups were selected using the categorical variables for decile of personal income and decile of (modified OECD) equivalised household income. ABS provided the income ranges which correspond to each categorical income variable. For the equivalised variables, these income cut-offs were unequivalised, thus deriving raw income ranges which correspond to each category of the equivalised variable. These cut-offs differ by household composition (or income unit type) as defined by the relevant equivalence scale.

denote partial coverage for couples in 1995.[7] The box with a horizontal fill pattern for couples in 2001 represents an income range that was not used in the main analysis, but the results are not sensitive to its inclusion as reported in the Appendix.

Figure 3 Ideal and proxy income thresholds for selection of treatment and comparison groups (2001 AU $ per week)





[7] This results from two factors. One is the addition of two incomes presented as ranges (one for each member of the couple). The other is due to the process of converting equivalised income decile cut-offs into cash income. The equivalence scale used by ABS in 1995 is a simplified version of the Henderson Scale, which differs according to labour force status (but not age).

Table 1 shows the resulting sample size by year and group. Whilst the overall sample is large, the number of observations in the treatment group is considerably smaller. Table 1 also shows the percentage of people in each group who have a health care card, suggesting that the policy change is strongly correlated with health care card status. Health card coverage increased by 31 percentage points between 1995 and 2001 and by 36 percentage points between 1995 and 2004-05. Among the comparison groups, differences in concession card coverage over time were no greater than 4 percentage points.

However, 43.2% of the treatment group had a health care card in 1995. This implies that a large proportion of the treatment group 'received treatment' prior to the intervention and are hence misclassified. This is not a problem for the IV approach under the assumption that the factors causing misclassification in 1995 affect the same proportion of the treatment group in other years. Thus the reasons for misclassification require scrutiny. The majority (82.2%) of this misclassified subgroup received a public pension and were hence eligible for a pensioner concession card or a veterans' treatment entitlement card.[8] The definition of income for the purpose of pension eligibility is different to the definition in the NHS.[9] Given that the number of pensioners dominates the number of higher income older people, it is not surprising that a large proportion of the higher income group consists of pensioners affected by this definitional issue. It is also possible (though not inevitable) that income is misreported for some of these pensioners. Also, some categories of veteran's pensions are not subject to the same income tests, though the type of pension is not revealed in the 1995 dataset. The remaining 17.8% of this subgroup (7.7% of the total 1995 treatment group) can be explained by a combination of other factors. Some may be CSHC holders, affected by similar income definition issues as the age pensioners discussed above. For others, pension

[8] It would hence seem natural to exclude pensioners from the treatment group in all years. However, much of the treatment group samples in 2001 and 2004-05 are (legitimately) pensioners. This is because the income eligibility threshold for the age pension was increased in July 2000. Specifically, the taper rate was reduced from 50% to 40%, thus increasing the range of incomes that qualify for a part rate of the age pension. Thus it is impossible to apply a consistent exclusion rule across samples to address this issue. Again, this is not an issue for the analysis under the assumption that the misclassification is consistent across years.

[9] As noted above, NHS records 'current' income. In principal, the income test for the age pension is an annual income test, though income over shorter periods (such as 13 weeks) may be assessed for people with irregular income (see Australian Government, 2007b: Section 4.3.1.20).

receipt or health card status may be misrecorded. All of the explanations proposed so far are likely to affect the treatment group in each of the three surveys. There is one potential factor, however, which affects only the treatment group in 1995. The health care card flag includes safety net concession cards in NHS 1995, but not in other years (Australian Bureau of Statistics, 1996: 92). This may result in the proportion of people affected by the CSHC reform to be underestimated and hence the magnitude of IV elasticity estimates to be overestimated. However, a number of factors suggest that this is unlikely to be major factor. As discussed above, this issue is one of several competing explanations for the misclassification of just 7.7% of the treatment group. ABS states that 'there was significant underreporting of safety net cards' (Australian Bureau of Statistics, 1996: 92). Furthermore, since the survey was conducted throughout the calendar year, the number of people to have reached the safety net threshold before the time of interview is likely to be small, though data are not available to establish this. Finally, safety net card holders, by definition, have had high drug consumption in the calendar year prior to interview. But there is no evidence of a greater difference in pharmaceutical consumption between card holders and non-card holders in 1995 than in other years. This is shown in the Appendix (Figures 12, 13 and 14).

Table 1 Sample size and proportion with health care card by year and group

Sample size	1995	2001	2004-05	Total
Treatment group	270	224	298	792
Comparison Group 1 (poorer)	3,896	2,263	2,711	8,870
Comparison Group 2 (younger)	700	583	678	1,961
Total	4,866	3,070	3,687	11,623
Percentage with Health Care Card				
Treatment group	43.2%	73.9%	78.9%	68.8%
Comparison Group 1 (poorer)	94.0%	97.2%	98.0%	96.4%
Comparison Group 2 (younger)	5.9%	8.5%	8.1%	7.8%

The measure of consumption used in the analysis is the number of PBS medications taken in the previous two weeks for selected conditions.[10] Each of the three surveys contains such

[10] This variable might not capture all possible behavioural responses to price. A change in price *ceteris paribus* could be associated with changes in frequency or dosage of consumption within a given fortnight. It may also

data. The generic names of drugs taken are provided in each file. The data do not include an indicator for whether the drug was purchased through the PBS. Each generic drug name was manually checked against the PBS Schedule operating at the time (Australian Government, various years). It is assumed that drugs appearing on the Schedule were purchased through the PBS. In each survey, the drugs recorded were as reported by the respondent. Interviews took place in respondents' homes and respondents 'were encouraged to bring out the medication packets, bottles, etc to assist them and interviewers in recording complete and correct details' (Australian Bureau of Statistics, 2003). Whilst the ABS states that this did not always occur, such procedures help minimise measurement error.

Data on pharmaceutical consumption were collected differently in 1995 to the other years. In 1995, data were collected on all medications taken (to a maximum of twelve). In 2001 and 2004-05, data were only collected for medications taken for specific conditions. In 2001, the conditions were asthma, heart and circulatory conditions, diabetes and high sugar levels (as well as for cancer and mental wellbeing, but generic drug names were not provided). A maximum of three drugs were recorded for each condition. Up to three heart and circulatory conditions were recorded. The data for 2004-05 were collected similarly to that of 2001 with the following exceptions. Medications for arthritis and osteoporosis were recorded, while cancer was omitted. Generic drug names were provided for medications taken for mental well-being. Up to five medications were recorded for mental health and wellbeing, while a maximum of three were recorded for other conditions, as for 2001.

For 2001 and 2004-05, a comparable variable was created which records the number of PBS drugs taken for asthma, heart and circulatory conditions, diabetes and high sugar levels. These medications account for approximately 41% of PBS prescriptions and 53% of the corresponding benefits in 2001, as recorded by Medicare Australia (Australian Government, 2006).[11] The ABS has classified medications into types that are commonly used for specific conditions, based on the WHO Anatomical Therapeutic Chemical Classification (Australian

induce the use of out-of-date or substitute medications, without necessarily impacting on the dependent variable. If so, the effect of price on quantity demanded may be underestimated. This would result in an underestimation of the magnitude of the estimated elasticity.

[11] The corresponding proportion calculated for the population of interest is not available. However, given the set of conditions covered, it is likely to be considerably higher than the 41% across all age groups.

Bureau of Statistics, 2006: Appendix 4). Using this classification, medications that are commonly taken for the conditions listed above have been included in the corresponding variable for 1995. Note that this method does not ensure that the same drugs are included in each year. This is justified on the basis that treatments for these conditions change over time. For the estimates to be unaffected by the methodological differences between years, it is sufficient to assume that they affect the treatment group and comparison groups equally.

The pharmaceuticals that are included in the measure are almost exclusively for the treatment of chronic conditions. Almost all the medications fall into the category of 'essential' medicines used in other studies (McManus *et al.*, 1996; Tamblyn *et al.*, 2001). One might hypothesise that the price elasticity of demand is smaller for essential medicines than other drugs. However, the evidence provides little support for this hypothesis. There was no difference between essential and less-essential medicines in the response to a price change amongst older people in Australia (McManus *et al.*, 1996) and only a small difference in Canada (Tamblyn *et al.*, 2001). The dependent variable is dominated by heart and circulatory drugs, which make up 82% of the total volume of drugs recorded for the treatment group across years. Whilst it would be desirable to conduct analyses of various sub-groups of drugs, the data are not rich enough to warrant these.

Table 2 shows descriptive statistics for each group in the sample. In each year, average PBS drug consumption was highest for comparison group 1 and lowest for comparison group 2. The number of PBS drugs taken was very similar in 1995 and 2001, and the difference is not significantly different for any group. However, the recorded number of PBS drugs was considerably lower in 2004-05 for each group, despite similar methods being used in the last two surveys. This is explained by differences in the medication classification provided on the files. In 2004-05, a greater proportion of medications were grouped into residual categories, making it difficult to determine which were PBS-listed. Such medications were not included in the 2004-05 count. There is a notable difference over time in the sex composition of the treatment group. Females accounted for 56% of the group in 1995, decreasing to 40% in 2004-05.

Table 2 Summary statistics (mean and standard deviation) by year and group

	mean (standard deviation)		
1995	Treatment Gp	Comp Gp 1	Comp Gp 2
Number of PBS drugs taken in previous two weeks	1.167 (1.306)	1.256 (1.468)	0.517 (0.921)
Age	71.68 (4.196)	73.04 (4.112)	55.96 (3.597)
Female	0.556 (0.498)	0.597 (0.491)	0.428 (0.495)
Personal income (2005 AU$ / week '00s)	5.346 (2.316)	2.133 (0.928)	6.044 (2.661)
Married	0.648 (0.478)	0.520 (0.500)	0.643 (0.480)
High blood pressure	0.379 (0.486)	0.391 (0.488)	0.237 (0.426)
Cholesterol	0.142 (0.350)	0.108 (0.310)	0.135 (0.342)
Diabetes	0.066 (0.249)	0.088 (0.284)	0.038 (0.191)
Asthma	0.080 (0.272)	0.083 (0.277)	0.076 (0.265)
Self Assessed Health - very good	0.284 (0.452)	0.222 (0.415)	0.380 (0.486)
Self Assessed Health - good	0.332 (0.472)	0.306 (0.461)	0.322 (0.468)
Self Assessed Health - fair	0.158 (0.366)	0.260 (0.438)	0.127 (0.334)
Self Assessed Health - poor	0.083 (0.277)	0.120 (0.325)	0.022 (0.146)
Health Care Card	0.432 (0.496)	0.940 (0.237)	0.059 (0.236)
2001	Treatment Gp	Comp Gp 1	Comp Gp 2
Number of PBS drugs taken in previous two weeks	1.036 (1.339)	1.306 (1.513)	0.505 (1.066)
Age	72.63 (4.330)	73.35 (4.139)	55.58 (3.561)
Female	0.492 (0.501)	0.562 (0.496)	0.426 (0.495)
Personal income (2005 AU$ / week '00s)	5.779 (2.321)	2.354 (1.242)	6.434 (3.045)
Married	0.602 (0.491)	0.570 (0.495)	0.719 (0.405)
High blood pressure	0.390 (0.489)	0.402 (0.490)	0.191 (0.393)
Cholesterol	0.173 (0.379)	0.198 (0.399)	0.124 (0.330)
Diabetes	0.080 (0.272)	0.114 (0.318)	0.054 (0.226)
Asthma	0.064 (0.245)	0.094 (0.291)	0.073 (0.260)
Self Assessed Health - very good	0.277 (0.449)	0.204 (0.403)	0.332 (0.471)
Self Assessed Health - good	0.288 (0.454)	0.344 (0.475)	0.332 (0.471)
Self Assessed Health - fair	0.208 (0.407)	0.238 (0.426)	0.155 (0.362)
Self Assessed Health - poor	0.041 (0.200)	0.112 (0.315)	0.045 (0.208)
Health Care Card	0.739 (0.440)	0.972 (0.164)	0.085 (0.279)
2004-05	Treatment Gp	Comp Gp 1	Comp Gp 2
Number of PBS drugs taken in previous two weeks	0.880 (1.105)	1.076 (1.191)	0.393 (0.749)
Age	72.44 (4.408)	73.50 (4.177)	56.46 (3.614)
Female	0.398 (0.490)	0.568 (0.495)	0.441 (0.497)
Personal income (2005 AU$ / week '00s)	6.219 (2.708)	2.375 (0.854)	6.671 (2.652)
Married	0.663 (0.473)	0.559 (0.497)	0.635 (0.482)
High blood pressure	0.357 (0.480)	0.423 (0.494)	0.208 (0.406)
Cholesterol	0.185 (0.389)	0.232 (0.422)	0.104 (0.305)
Diabetes	0.089 (0.286)	0.152 (0.359)	0.051 (0.221)

Asthma	0.108 (0.311)	0.097 (0.296)	0.080 (0.272)
Self Assessed Health - very good	0.311 (0.464)	0.236 (0.425)	0.401 (0.490)
Self Assessed Health - good	0.313 (0.464)	0.326 (0.469)	0.295 (0.456)
Self Assessed Health - fair	0.138 (0.345)	0.224 (0.417)	0.101 (0.301)
Self Assessed Health - poor	0.050 (0.219)	0.120 (0.325)	0.040 (0.196)
Health Care Card	0.789 (0.409)	0.980 (0.140)	0.081 (0.273)

4.3 Common trends?

The key assumption in a difference-in-difference approach is that of a common trend between groups. The steady increase in aggregate health care consumption is largely driven by technological change (Manning *et al.*, 1987). I propose that technological progress is more likely to affect consumption multiplicatively rather than additively. Over a given period, consumers with health problems might be affected by several technological advances, whilst those that are healthy might be affected by none since they do not require health care intervention. If so, a common trend in logs is a more appropriate assumption than a common trend in levels.

One way of comparing the validity of the competing assumptions about trends is to analyse consumption over a period that does not include the policy intervention. I consider changes in consumption for each of the groups between 2001 and 2004-05, the two post-intervention samples. The only notable change over this period was a 21% increase in the co-payment in January 2005, which affected both general and concession patients.

Using the data in Table 2, it can be shown that there is no significant difference in the absolute or relative level of consumption between 2001 and 2004-05 for any group. Such a comparison, however, does not control for possible difference in characteristics between years. A difference-in-difference approach in a regression framework facilitates similar comparisons which do control for changes in the characteristics of groups. To test the hypothesis of a common trend in levels, consumption was modelled using OLS for each group separately. The key variable of interest is a year dummy. Control variables include age, age squared, sex, personal income, personal income squared, marital status, self assessed health and binary flags for high blood pressure, high cholesterol, diabetes and asthma. To test the hypothesis of a common trend in logs, it is not possible to take the

logarithm of the dependent variable due to the large number of zeros. Instead, negative binomial regressions were estimated (to be discussed further in the following section). Since the negative binomial regression is estimated with a log link, it models the multiplicative effect of the covariates. The estimated effects of the year dummy on consumption are shown in Table 3. The parameters of interest, labelled 'Pct change' are non-linear transformation of the estimated coefficients of year: e^{β}-1. As in the raw comparisons, the effect of year is not significantly different between the treatment group and either comparison group using either method. In the linear model, however, the effect is significantly larger ($p<0.001$) for the low income comparison group (which has the highest average consumption) than the young comparison group (which has the lowest consumption). This is not the case in the negative binomial model. This result combined with the proposition above provides some justification for the assumption of common trends in logs. This is the assumption that is made in the preferred models, but sensitivity of the results is tested to the alternative assumption of common trends in levels. The estimated elasticity is not sensitive to this assumption.

Table 3 Modelled effect of year on consumption by group

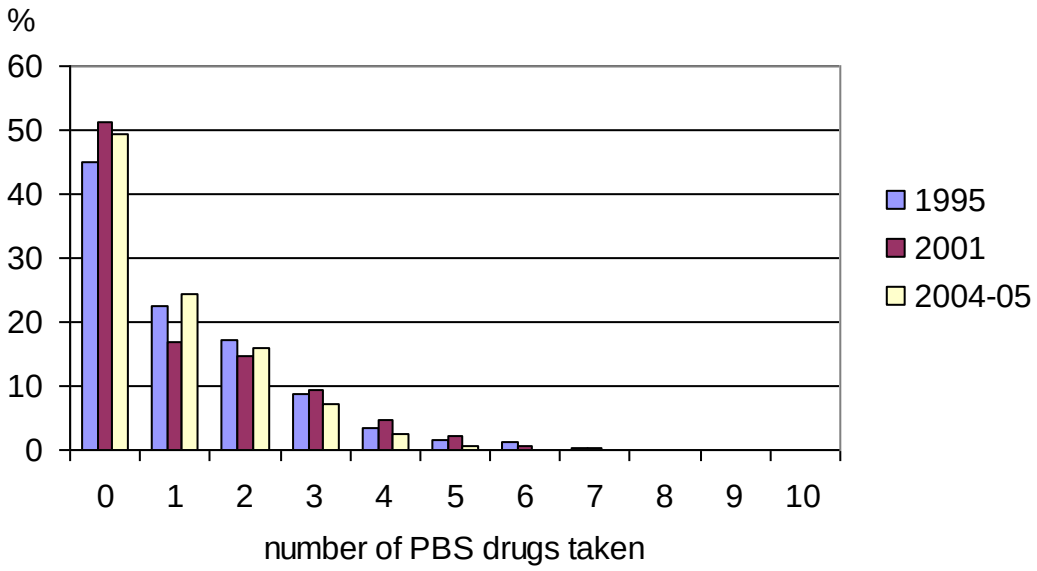
OLS	Coefficient	Robust SE		
Comp Gp (low income)	-0.324	0.033		
Treat Gp	-0.194	0.083		
Comp Gp (younger)	-0.113	0.039		
NegBin	Coefficient	Robust SE	Pct change	Delta SE
Comp Gp (low income)	-0.251	0.029	-22.16%	2.28%
Treat Gp	-0.177	0.106	-16.20%	8.88%
Comp Gp (younger)	-0.223	0.106	-19.97%	8.46%

4.4 Model Specification

The dependent variable is a highly skewed count variable. Figure 4 shows its distribution by year. One reason to prefer a count data model to the linear model shown in equation (5) is a gain in efficiency. The residuals in an OLS regression estimated on count data are likely to be non-normal and heteroskedastic. OLS is a consistent estimator under such conditions.

However, by utilising additional information about the likely distribution of the residuals, count data models can be more efficient. A second advantage to count data models is that the parameters usually estimate the multiplicative effect of the explanatory variables on the dependent variable. This allows the implementation of the common trend in logs assumption. This assumption cannot be implemented in a linear regression model unless the dependent variable has no zero values.

Figure 4 Distribution of Dependent Variable by Year, All Groups (% within each year)



Two count data models are considered, the Poisson Regression Model (PRM) and the Negative Binomial Regression Model (NBRM), following Cameron and Trivedi (1998) and Long (1997).

In the PRM, the dependent variable y_i, conditional on covariates x_i, is assumed to be Poisson distributed:

$$f(Y_i = y_i \mid x_i) = \frac{e^{-\mu_i}\mu_i^{y_i}}{y_i!}, \qquad y_i = 0, 1, 2, \ldots$$

The conditional mean is parameterised as a log-linear function of covariates:

$$E(y_i \mid x_i) = \mu_i = \exp(x_i^{'}\beta)$$

and by the Poisson process, the conditional variance is equal to the conditional mean.

The NBRM extends the PRM, by allowing for overdispersion. In other words it allows the conditional variance to exceed the conditional mean. This extension can be motivated by unobserved heterogeneity. In this case, the conditional mean is a random variable.

$$E(y_i \mid x_i) = \tilde{\mu}_i = \exp(x_i^{'}\beta + \varepsilon_i)$$

where ε_i has a mean of zero and hence $E(\tilde{\mu}_i) = \mu_i$ The conditional variance is greater than the expected conditional mean and is parameterised as:

$$Var(y_i \mid x) = \mu_i + \alpha\mu_i^2$$

where α is estimated by the model. The PRM is thus a special case of the NBRM where α is equal to zero. This restriction can be tested using a Wald test.[12] The PRM is consistent with distributional misspecification such as an inappropriate assumption of equidispersion. The NBRM (and OLS) are also consistent with distributional misspecification and robust standard errors can be calculated which are analogous to Huber-White (Sandwich or robust) standard errors in the linear model.

In the two-period one comparison group NBRM, the model is specified as:

$$u_i = \exp(b_0 + b_1 t + b_2 G_i + b_3 H_i + \beta X_i + \varepsilon_i), \qquad (6)$$

where t $\in$(0,1),

G = 1 if person i has income and age such as to be affected by the policy change; zero otherwise

H = 1 if person i has a health card; zero otherwise

[12] The Wald statistic is calculated as $(\hat{\theta} - \theta)^2 / \mathrm{var}(\hat{\theta})$ and follows a Chi Square distribution with one degree of freedom.

X is a vector of controls including sex, age, age squared, marital status, personal income, personal income squared, self assessed health and binary indicators for high blood pressure, cholesterol, diabetes and asthma as long-term health conditions.

If H is exogenous, consistent estimates of b_3 can be generated by estimating (6) by Maximum Likelihood. If H is endogenous, however, such estimates will be inconsistent and instrumental variable methods are necessary, though they come at a cost to efficiency.

Models where endogeneity is assumed are estimated by the Two-Step (efficient) Generalized Method of Moments (GMM), using the moment conditions proposed by Mullahy (1997) for count data models. Mullahy's approach is to treat the residuals as multiplicative rather than additive, thereby treating observed and unobserved characteristics symmetrically. See Davidson & MacKinnon (1993: Chapter 17) for a detailed introduction to GMM. In addition to Mullahy, see also Windmeijer & Santos Silva (1997) for a discussion of additive versus multiplicative residuals in count data GMM models.

The exogenous policy change (measured by the interaction of G and t) is an instrument for H. To test the endogeneity of H, a Hausman test is performed (Hausman, 1978; Greene, 2003). This tests for a significant difference in the estimated coefficients of two competing specifications, where one is efficient and the other is consistent under less restrictive assumptions. In this case, it is used to test for differences between the GMM model with H treated as endogenous and the corresponding model with H treated as exogenous. The test statistic has a χ^2 distribution with degrees of freedom equal to the number of hypothesised endogenous regressors (1 in this case).

The statistical power of such endogeneity tests depends on the strength of the instruments. Statistically significant evidence for bias may not be found given weak instruments or small sample size. Nakamura and Nakamura (1998) caution against mechanical use of such tests as a decision rule for whether to use IV. Taking this on board, both IV and non-IV models are estimated even if the Hausman test does not find statistically significant bias due to endogeneity. There is indeed an *a priori* reason to expect endogeneity. The CSHC is taken up by a subset of eligible people. Conditional on covariates, it seems probable that high-end consumers of pharmaceuticals are more likely to take-up the card than others, since they have the most to gain from a concession. As a result, CSHC holders will have higher

consumption than implied by the price effect alone. Thus non-IV estimates of elasticity will be biased downwards (more negative). In the IV model, take-up is instrumented by eligibility. It is hypothesised that IV estimates will be higher (less negative) than non-IV estimates.[13]

The endogeneity test is identified through a policy change that only affects the 'treatment group', which has been defined as the set of people whose income and age suggest that they were affected by the policy reform. For this reason, the sample is restricted to the treatment group (*G*=1) in the non-IV models. Under this restriction, the non-IV estimates have a slightly different interpretation to the IV results. They are estimates of the effect of the card on those who have taken it up (the estimated average effect of the treatment on the treated). The IV estimates, on the other hand, provide estimates of the Local Average Treatment Effect. This is the effect of the card amongst those who have taken it up as a result of the policy change. Since some card-holders in the 'treatment group' already had the card prior to the treatment, the two sets of estimates are conceptually different.

The model is extended to enable two comparison groups and/or three time periods. In the two-comparison group model, *G* is replaced with two group dummies. In the three-period model, *t* is replaced by two time period dummies. In either case, the interaction term *Gt* is replaced by a dummy variable that equals one for the treatment group after the policy change, and zero otherwise. The set of instruments is also enriched by interacting *Gt* with each exogenous regressor in the model.

Note from (6) that $E(C \mid H=1) = \exp(b_3)E(C \mid H=0)$. Substituting, the first term of equation (1a) simplifies to a non-linear transformation of the estimated coefficient of *H*:

[13] It is also possible that income is endogenous. Some people may have changed their income earning behaviour in response to the policy change. If so, eligibility for the card (and the instrumental variable) would be endogenous. Any such endogeneity would result in high users moving into the treatment group post treatment and the effect of price on consumption would be overestimated. This would mean that the estimated elasticity is biased downwards and the unbiased estimate is higher (less negative) than estimated. However, the behavioural response is unlikely to be great since the value of the concession is not large. For an average consumer in the treatment group, the value of the concession is equal to approximately 1% of income. Even for a consumer with ten times the average consumption, its value is only 3% of total income (because of the safety net).

$$\frac{\exp(b_3)C_{H=0} - C_{H=0}}{(\exp(b_3)C_{H=0} + C_{H=0})/2} = \frac{\exp(b_3) - 1}{(\exp(b_3) + 1)/2} \quad (7)$$

The numerator of (7) also represents the percentage change in consumption associated with the policy change, for those who took up the card.

The NBRM specified in equation (6) has an analogous linear model:

$$C_i = b_0 + b_1 t + b_2 G_i + b_3 H_i + \beta X_i + \varepsilon_i \quad (8)$$

This model is used to test the sensitivity of the results to an assumed common trend in levels rather than logs. The first stage of the linear IV model is

$$H_i = \alpha + \gamma G_i t + \phi t + \varphi G_i + \delta X_i + \lambda G_i t X + \nu_i \quad (9)$$

In the linear model, the first term of equation (1a) is equal to:

$$\frac{b_3}{(E(C \mid H = 0) + b_3)/2} \quad (10)$$

Where $E(C \mid H = 0)$ is calculated as the average predicted value for the treatment group post-treatment, conditional on H=0.

Standard errors in all models are robust (sandwich/White) errors.

4.5 Propensity Score Matching

Matching is a technique for selecting a comparison group that resembles the treatment group as closely as possible on observable characteristics. The assumption underpinning the matching approach is that treatment is uncorrelated with the outcome variable, conditional on observed characteristics. This is sometimes referred to as 'selection on observables' or 'conditional independence'. This approach requires a common support (a common range of probabilities of treatment given observable characteristics) to the treatment and comparison groups. It is sufficient to match on the probability of treatment as a function of observable characteristics (Rosenbaum & Rubin, 1983). Such an approach is known as Propensity Score Matching (PSM). A key strength of matching approaches is that they do not impose parametric assumptions on the relationship between the outcome variable and the regressors.

In this paper, a set of results are produced using PSM. As in the non-IV regressions described above, the sample is restricted to the set of people whose income and age suggest that they were affected by the CSHC reform. Within this sample, the 'treatment group' is redefined as the set of people who hold a concession card. The comparison group is the set of people who do not have a concession card. Each observation in the treatment group is matched with its five 'nearest neighbours' (with replacement) from the comparison group.[14] These are the observations that have the smallest absolute difference between their odds ratios and that of the observation in the treatment group. The matched value of the outcome variable is the average of these five observations. Observations in the treatment group that fall outside of the common support are excluded. This approach does not exploit the CSHC natural experiment, just as the non-IV models do not exploit it. The approach is justified if there is no evidence found for endogeneity of health care card take-up.

The issue of how to account for complex survey data in PSM has been given little attention in the academic literature. Complex survey data are characterised by unequal probability of selection. It would seem likely that an unweighted average effect of the treatment on the treated may not be representative of the population from which the sample is drawn. It is less clear, however, whether the propensity score model (the probit stage) should be weighted. Zanutto (2006) suggests that the sensitivity of results to both weighted and unweighted approaches should be shown. The main approach used in this paper is to calculate a weighted estimate of the treatment effect. Only the weights that correspond to treatment group observations are used. These are applied to the treatment group outcomes, as well as to the outcomes that are matched to each treatment group observation. The propensity score is calculated using an unweighted probit regression. Sensitivity is tested to the use of sample weights in both stages.[15]

[14] The choice of five neighbours is common in applied evaluations. Ravallion (2007: 28) suggests that using five neighbours produces more robust estimates than using one neighbour. The estimates are similar when a nearest one neighbour approach is used, but less precise, as shown in the Appendix.

[15] My approach is based on the module written by Leuven and Sienesi (2003). I have made simple modifications to enable the use of sample weights.

4.6 Other Methods

The approaches outlined above were selected from a suite of non-experimental economic evaluation methods (see reviews by Blundell & Costa Dias, 2000; Cobb-Clark & Crossley, 2003; Ravallion, forthcoming). Two other methods that were considered were regression discontinuity and a Wald estimate that uses PSM & difference-in-difference. The use of administrative data was also considered.

Regression discontinuity analyses identify causal impacts by exploiting program eligibility rules. This method is particularly useful if eligibility is defined according to a point on an observable exogenous variable, where people above (below) are eligible and those below (above) are ineligible. The causal effect of the program is identified by comparing the outcomes of people above the discontinuity to those below the discontinuity. The relationship need not be deterministic, and a Wald estimator can be used if the probability of treatment changes at the point of the discontinuity (Hahn *et al.*, 2001).

Two applications of the regression discontinuity method were considered. Eligibility is defined according to two continuous variables: age and income. One approach would be to exploit the income eligibility rule as a discontinuity, whilst limiting the analysis to those of eligible age. There are several reasons why this was not pursued. It was argued above than endogenous income is unlikely to be a major issue for the analysis. However, it may be more of an issue at incomes that are close to the eligibility threshold. Secondly, the publicly available data generally provide income as a categorical variable, making it difficult to select people immediately above and below the threshold, or to identify the underlying relationship between income and consumption near the discontinuity. The 2004-05 file is an exception, in which income is coded continuously. However, the threshold is very high in the income distribution. Only 124 observations had income above the threshold, 44 of whom were covered by a health card or did not know if they were covered.

An alternate regression discontinuity approach would be to exploit the age eligibility rule, whilst limiting the analysis to those of eligible income. The problem here is that the discontinuity (65 years for males; 62 years for females) coincides with eligibility for the age pension, the age at which many people retire. For many people, the discontinuity would

coincide with changes (reductions) in income. It is therefore unlikely that the two groups are comparable.

Blundell & Costa Dias (2000) propose an approach that combines PSM with difference-in-difference. Doiron (2004) used this approach to examine labour supply with repeated cross-sectional data and it could also be used with the CSHC natural experiment. It would be necessary to incorporate a Wald-estimate adjustment, since the policy change does not determine health care card status (Wald, 1940). This approach has not been implemented in the current investigation, but it is a possible extension in future research.

Consideration was also given to the use of administrative data (from Medicare Australia) to supplement the survey data. Such data do not include information on income, so it would be difficult to identify people whose eligibility was actually affected by the policy change. An ideal approach would be to match administrative data with (preferably longitudinal) survey data. This would require the consent of respondents. I do not know of any survey data where consent was given over the relevant period. Consent has been obtained for respondents of the Australian Longitudinal Study on Women's Health (Women's Health Australia, 2007). However, the matching to administrative data only commenced in 2002, and in any case the survey contains minimal information on income.

4.7 Summary of Methods

To briefly summarize, I estimate the price elasticity of demand for pharmaceuticals amongst high income older people by separately considering the effect of the CSHC on consumption and on price. I estimate the effect on consumption using a range of methods. I exploit the natural experiment to conduct linear and non-linear IV difference-in-difference regressions on repeated cross sectional survey data. In these models, the effect is identified by changes in consumption by the treatment group relative to that of the comparison groups. The IV approach adjusts for incomplete take-up as well as the misclassification of observations into the treatment group, under the assumption that this is constant across years. I use the same instrument to conduct tests of bias due to the endogeneity of health care card take-up. Next, assuming exogeneity of take-up, I implement a series of cross-sectional models, with the sample restricted to the population of high income older people. In these cross-sectional models, the effect is identified by the difference in consumption between people

with and without health care cards. The effect of the CSHC on the average marginal price of pharmaceuticals is separately estimated using a conservative approach described and implemented in the next section.

5. The effect of the CSHC on price

This section discusses the estimated effect of the CSHC on the price of PBS medications. More precisely, the issue of interest is the price change that corresponds with the modelled changes in consumption. The discussion begins with the price change that corresponds with the IV models, followed by the price change that corresponds to the non-IV models.

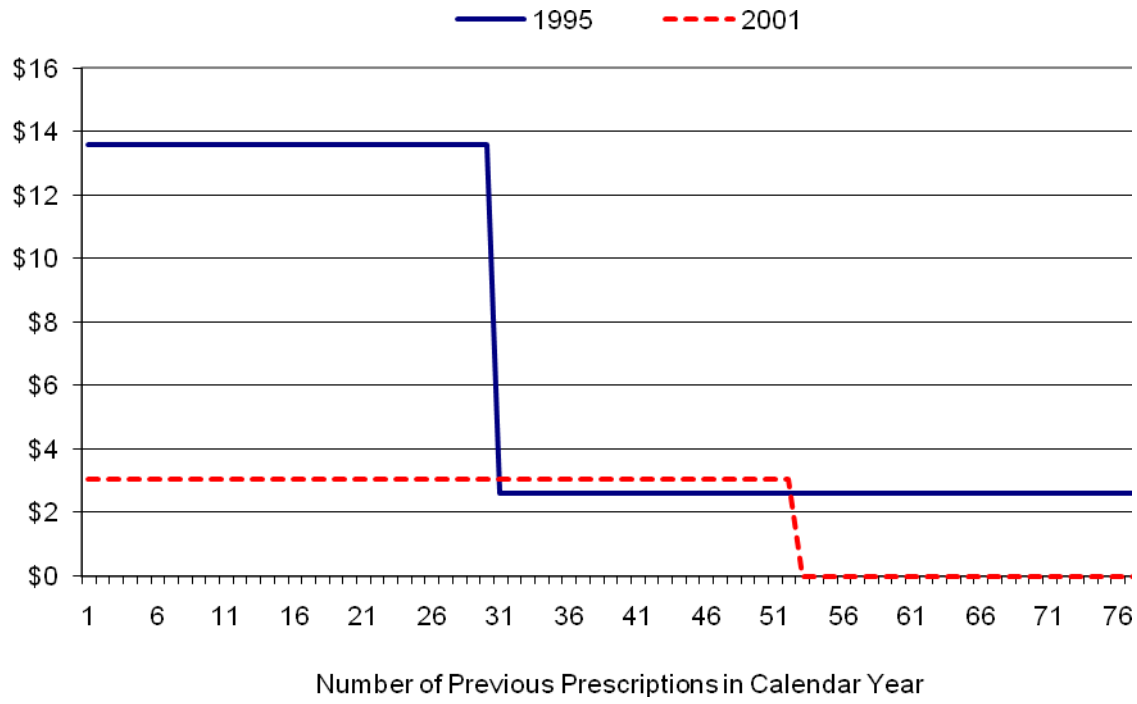
In the IV models, the demand response is identified through change in consumption of those who took up the CSHC (between 1995 and 2001 or 2004-05), relative to that of the comparison groups. The corresponding price change is that of the average marginal price (at the time of the NHS interview) of listed medications for those that took up the CSHC, relative to that of the comparison groups. Identification is complicated by two factors. One is that some drugs are priced at below the notional general co-payment amount. Secondly, the price schedule is non-linear due to the PBS safety net. The price is lower after a consumer reaches a given out-of-pocket expenditure in the calendar year.

The first issue can be dealt with relatively easily. The out-of-pocket price for listed medications is recorded in the schedule operating at the time (Australian Government, various years). The out-of-pocket price was matched to the drugs taken by members of the treatment group (excluding those with a concession card) in 1995. The average of these prices (weighted by consumption) is $13.59 and this is taken as the unit price of drugs for patients who had not reached the safety net threshold. No drugs are priced below the safety-net price for general patients, so the safety copayment is taken as the marginal price for people beyond the safety net threshold.

Figure 5 compares the price schedule facing the treatment group in 1995 and 2001 in real terms. The first 30 prescriptions were 78% cheaper in 2001 than in 1995. The next 22 prescriptions, however, were 17% more expensive. Any further prescriptions were 100% cheaper (free) in 2001. For comparison group 1 (predominantly card holders), the

corresponding change was a 17% increase for the first 52 prescriptions and no change thereafter. For comparison group 2 (almost no card holders), the corresponding change was a 15% increase for the first 28 prescriptions, a more than five-fold increase for the next 8 prescriptions and a 17% increase thereafter.

Figure 5 Price Schedule for Treatment Group in 1995 and 2001 (1995 AU $ per prescription)



Source: Australian Government Department of Health and Ageing (2007)

To estimate the average marginal price change amongst the treatment group, it is necessary to estimate the proportions of respondents who fall into each of three components of the schedule (30 or less previous prescriptions; 31-52; 53 or more) at the time of interview. This distribution is not known, but can be bounded under plausible assumptions. Consider first the average number of prescriptions purchased by the treatment group in a calendar year. This is estimated using two methods. In the first method, the average number or prescriptions purchased per year by age pensioners was estimated using the 1998-99 Household Expenditure Survey. The ratio of the average number of prescriptions *taken in a fortnight* by age pensioners to people in the treatment group was calculated in NHS. This

ratio was assumed to also hold in annual drugs purchased. In the second method, the average annual number of prescriptions purchased per concession card holder was calculated using published data on the number of prescriptions (Australian Government, 2006) and the number of concession card holders (in NHS). Similarly to the first method, the ratio of the average number of prescriptions taken in a fortnight by all card-holders to people in the treatment group was calculated in NHS and assumed to also apply in the annual purchasing figures. For 1995, the two methods yielded estimates of 33.7 and 32.5 prescriptions per person, respectively. For 2001, the estimates were 36.5 and 28.9, respectively and for 2004-05 they were 39.1 and 27.7. Note that price is endogenous to consumption. To estimate the exogenous effect of the CSHC on price, it is necessary to assume a common level of consumption across the three years. Taking the average of all six estimates, I assume that people in the treatment group purchased an average of 33.1 PBS prescriptions per year.

I further assume that annual consumption is a random count variable with unknown dispersion. Figure 6 shows two such distributions. The first is a Poisson distribution, the second is a Negative Binomial distribution with α = 0.18 (see Cameron & Trivedi, 1998: equation (3.26) for the density function). These correspond to the possible distributions amongst respondents who were interviewed at the end of the calendar year. Corresponding distributions can be shown at any point of time in the calendar year. For example, the mean of the distributions is halved for those interviewed at the middle of the year. The average of all such distributions over the year results in the estimated distribution of PBS pharmaceuticals purchased in the calendar year prior to the time of interview (shown in Figure 7 with the thresholds that correspond to Figure 5.

Figure 6 Possible distributions for annual PBS drug purchases by treatment group

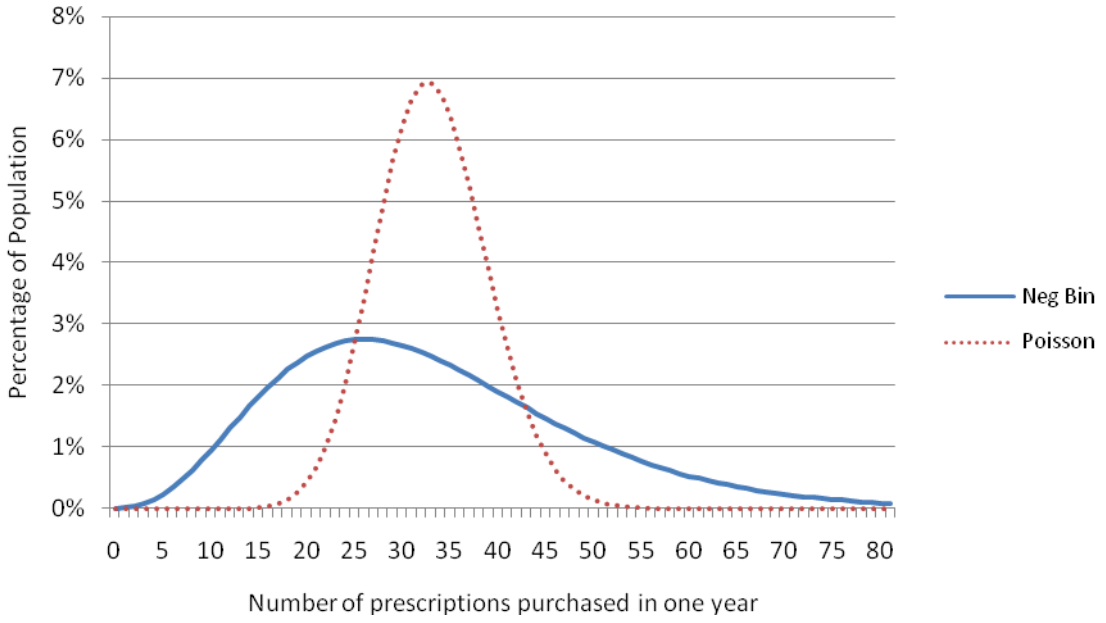
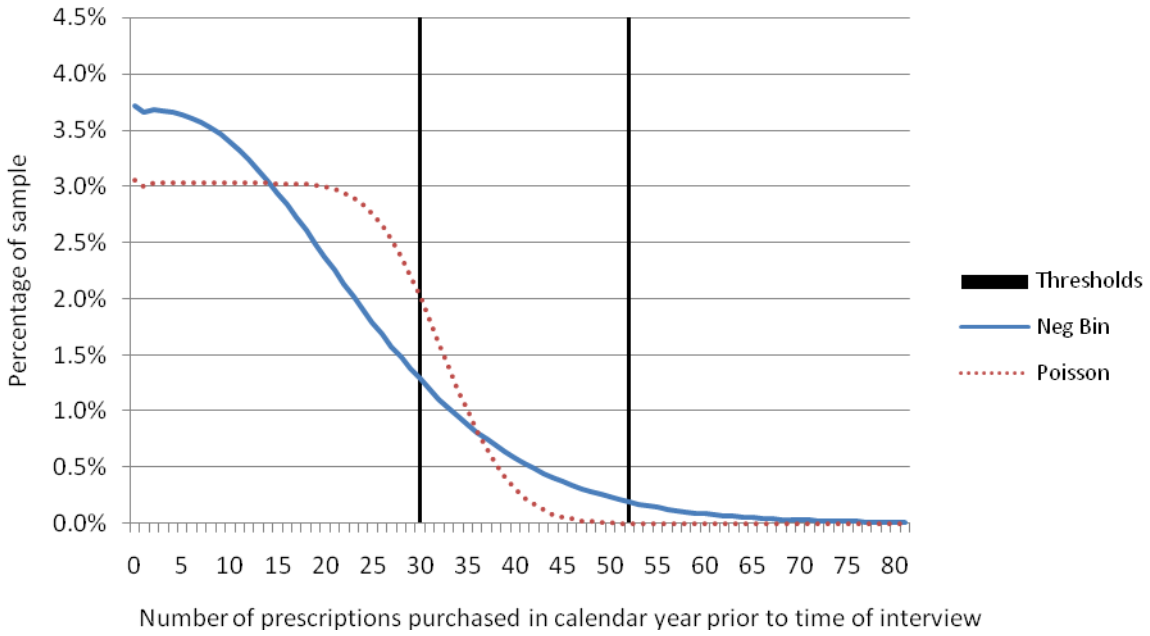


Figure 7 Possible distributions for number of PBS drugs purchased in calendar year up to time of interview by treatment group



As mentioned above, I do not have an estimate for the extent of overdispersion (α) in the distribution. However, the key features of the distribution are not sensitive to this parameter. Across all values of α, the maximum proportion of the distribution that falls in

the critical range of 31 to 52 prescriptions is 12.6%. For that group, the CSHC reform represents a 17% price increase. For the remainder of the treatment group, the CSHC represents a price fall of at least 78%. Using a similar process, it can be shown that the comparison groups experienced average price increases of at least 15% each. The modelled price change is the price change for the treatment group *relative* to the comparison groups. Thus for at least 87.4% of the treatment group, the modelled price change is a fall of 81%.[16] For the remainder of the treatment group, the modelled price change is an increase of less than 2%. Using these values, the weighted average of the second term in equation (1a) is evaluated to be –1.184. This is the value used in all elasticity calculations. The corresponding parameter is almost identical for the price change between 1995 and 2004-05, since all groups experienced virtually identical price changes between 2001 and 2004-05.

In the non-IV models and the PSM estimator, the effect of a concession card on consumption is identified in a more straightforward manner. It is measured by the discrepancy in consumption between people with cards and people without cards. The corresponding effect of a card on price is the discrepancy in average marginal price faced by the two groups at the time of interview. As above, price is endogenous to consumption and so consumption is assumed to be constant between groups for the purpose of estimating the average price discrepancy. The marginal price facing consumers was analysed in the same way as above for the IV models. A conservative value for the price difference was calculated by choosing α to minimize the magnitude of the second term in equation (1a). Under these assumptions, this term was evaluated to be –1.30 for 1995 and –1.31 for 2001 and 2004-05. For the pooled analysis, the average of these values (-1.31) was assumed. Thus the price difference in the non-IV models is slightly larger than that of the IV models.

As discussed above, a conservative approach was taken to estimate a lower bound for the modelled price change. However, this may be offset by two factors that lead to overestimation. As discussed earlier, out-of-pocket costs for PBS pharmaceuticals may exceed the notional co-payment by the Brand Premium or Therapeutic Group Premium if more expensive medications are chosen in place of cheaper medications judged to be of equal medicinal value. This could not be accounted for. Secondly, there may be non-

[16] (1-0.78)/(1+0.15)-1 = -0.81

monetary (time) costs associated with obtaining the medications, as well as (monetary and time) costs associated with seeing a General Practitioner to obtain a prescription. However, this might not be a major issue for several reasons. GP care is free for most Australians, especially people aged over 65 (at around 85%) (Abbott, 2005). For most medications, GPs can prescribe several courses (up to six) at one time. They can also prescribe as many different types of medication at one time as deemed appropriate. Furthermore, many people will have needed to see their GP in any case, for reasons other than to obtain the prescription. To the extent that price may be underestimated comparably for all groups, the relative price change associated with the policy change may be overestimated. If so, the magnitude of the elasticities may be underestimated.

It has not been possible to account for the impact of the non-linear price schedules on the 'effective price' facing consumers (Keeler *et al.*, 1977). The 'effective price' will be lower than the nominal price if the consumer expects to reach the threshold for lower prices in the future. The variables determining the effective price are the distance away from the threshold, the time remaining in the accounting period and the expected probability of future consumption. These factors have not been observed (except for time remaining in the accounting period, albeit poorly) and these may vary between individuals and groups. However, it is noted that every group in every year is subject to a non-linear price schedule, which might elicit this effect. The direction of the net effect is unclear.

6. Results

I begin by showing the results of the GMM IV model, followed by those of the non-IV Negative Binomial regression and the PSM results. Price elasticities that correspond to these results are then shown. The corresponding results for the linear regression models are shown in the Appendix.

Table 4 shows the estimated coefficients and standard errors for the GMM count data regression model (both comparison groups; all years), with and without controls. The p-value from the Hausman endogeneity test is shown in the last row. The test finds no

evidence for endogeneity in the model with full controls ($p > 0.31$).[17] There is evidence of endogeneity in the model with no controls ($p < 0.02$). This is because card take-up is correlated with the control variables (especially the health status variables as well as income and marital status). This is evidenced by the significant covariates in the first stage results in the linear IV model (see Appendix). Nevertheless, as discussed in methods, endogeneity is not ruled out, and results are shown for both IV and non-IV models.

The results show that the instrumented variable (hcard) is not statistically significant. The estimated coefficient of 'hcard' in the model with full controls implies that the policy change was associated with a –12% change in consumption amongst those who took up the card, with a wide 95% confidence interval (-73%, 50%). This wide confidence interval reflects the limited statistical power of the instrument.[18] Without controls, the corresponding estimate is a larger negative (–32%), suggesting that some of the apparent reduction in consumption is explained by the control variables. The control variables with the strongest effects are age, the specific health conditions and self assessed health.

The corresponding results from the linear IV model are shown in the Appendix. The estimated effect of a concession card on consumption is very similar to the count data model. The point estimate is –15% (95% CI: -65%, +34%). The same control variables are significant in both models, though age has a stronger effect in the GMM model.

[17] In the linear IV model, the p-value for the DWH endogeneity test is 0.23, supporting the lack of evidence for endogeneity (see Appendix).

[18] Statistically insignificant estimates were also estimated for the effect of the card on the number of doctors visits in the previous two weeks: 0% (95% CI: -74%, +74%) and on self assessed health (treated as a continuous variable): +7% (95% CI: -17%, +31%) (a negative indicates improvement in health).

Table 4 Estimated Coefficients From GMM Count Data Regression Model (Both Comparison Groups; All Years) With and Without Controls

	No controls			Full controls		
Variable	Estimated coefficient	Standard error		Estimated coefficient	Standard error	
group 1	-0.2485	0.1033	**	-0.0242	0.1020	
group 2	-1.2870	0.3822	***	-0.1163	0.3287	
Year 2001	0.0273	0.0384		-0.3555	0.0586	***
Year 2004-05	-0.1668	0.0378	***	-0.5550	0.0561	***
hcard	-0.3885	0.4585		-0.1246	0.3549	
age				0.1797	0.0842	**
age squared				-0.0009	0.0006	
female				-0.1385	0.0483	***
personal income				-0.0458	0.0385	
personal income squared				0.0043	0.0030	
married				0.0833	0.0463	*
high blood pressure				1.5750	0.0439	***
high cholesterol				1.1301	0.0547	***
diabetes				0.9424	0.0551	***
asthma				1.4223	0.0643	***
SAH - very good				0.5748	0.1040	***
SAH - good				0.8911	0.1030	***
SAH - fair				1.2416	0.1036	***
SAH - poor				1.4436	0.1170	***
constant	0.6164	0.4442		-9.8820	2.9880	***
p-value of Hausman endogeneity test		0.02			0.32	

* Statistically significant at the 10% level
** Statistically significant at the 5% level
*** Statistically significant at the 1% level

Table 5 shows the results of a non-IV negative binomial regression, where take-up of the health card is assumed to be exogenous. Health concession card remains statistically insignificant, but its coefficient is positive and the standard error is considerably smaller. It implies that the card is associated with a 16.3% increase in consumption (95% CI: -9.7%, 42.3%). As in the IV model, the health status variables are highly significant. Age and sex are not statistically significant, which may be due to the much smaller sample size. The corresponding results for the linear model are shown in Table 7 and are similar in substance. They imply a smaller consumption increase of 11.6% associated with the card (95% CI: -3.6%, 26.8%).

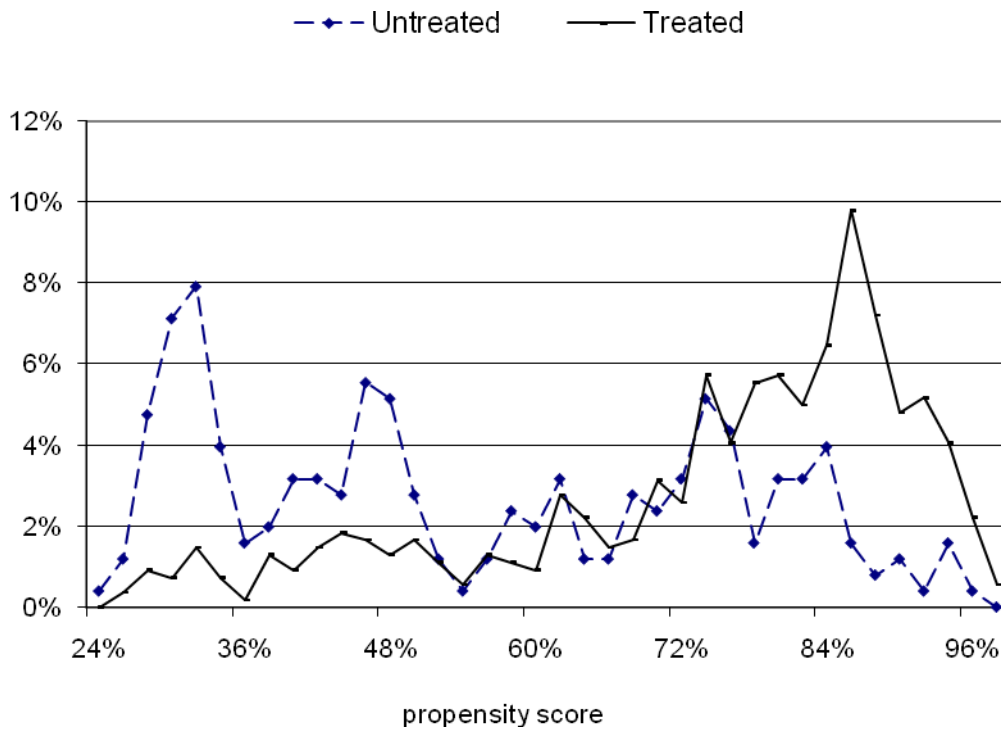
Table 5 Estimated Coefficients From Negative Binomial non-IV Regression Model (Both Comparison Groups; All Years)

	Estimated coefficient	Robust Standard error
Year 2001	-0.2794	0.1097**
Year 2004-05	-0.4743	0.1160***
health concession card	0.1509	0.1141
age	0.2116	0.5903
age squared	-0.0012	0.0041
female	-0.0313	0.0899
personal income	0.0442	0.0685
personal income squared	-0.0028	0.0056
married	-0.0115	0.0922
high blood pressure	1.1080	0.1076***
high cholesterol	0.5760	0.0944***
diabetes	0.5189	0.1149***
asthma	0.8384	0.1283***
SAH - very good	0.2328	0.1674
SAH - good	0.3755	0.1603**
SAH - fair	0.6246	0.1724***
SAH - poor	0.8188	0.2242***
constant	-10.156	21.437
alpha	0.000	0.000

* Statistically significant at the 10% level
** Statistically significant at the 5% level
*** Statistically significant at the 1% level

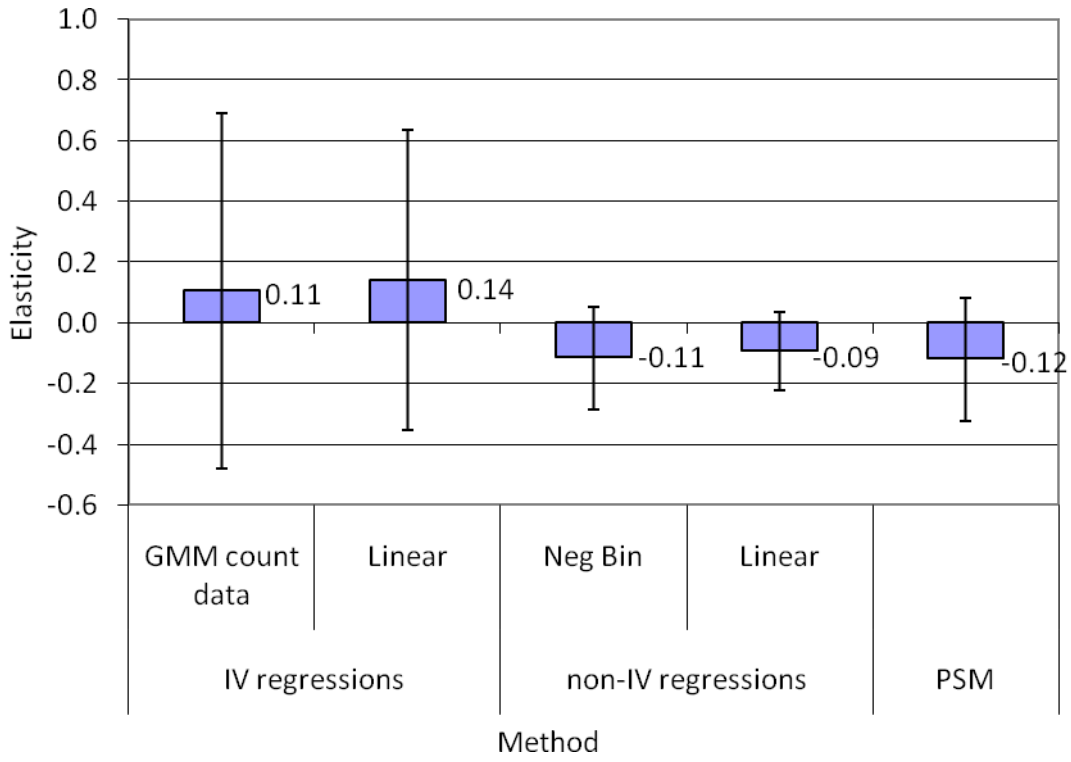
The PSM approach (with bootstrapped standard errors using 1000 replications) estimates an average treatment effect on the treated, which is directly comparable to the cross-sectional regression results. The resulting estimate is that the card is associated with a 17.0% (95% CI: -14.8%, 48.8%) increase in consumption. This is similar to the non-IV Negative Binomial regression result. The distribution of the estimated propensity score for treatment and comparison groups are shown in Figure 8. The density is not as dense at the upper end of the distribution for those without health cards as for those with cards (the treatment group). However, almost the entire range of scores is shared by the groups, with only 15 observations from the treatment group (2.8%) being outside the common support and hence excluded from the analysis.

Figure 8 Distribution of Estimated Propensity Score for Treatment and Comparison Groups



The main elasticity estimates are shown in Figure 9. The first is that of the preferred GMM count data IV regression model. This is a small statistically insignificant positive (+0.1), with wide confidence intervals. The second estimate is similar (+0.1) and corresponds to the linear IV model. Since the two estimates are similar, the results are not sensitive to the assumption of common trends in logs versus levels. These two estimates are not biased by endogenous take-up of the card. The following three estimates were derived from non-IV methods, which rely on cross sectional variation in consumption. The three non-IV estimates are very similar to each other (-0.1) with much smaller standard errors than the IV estimates. The non-IV estimates *might* be biased by endogenous take-up, but the Hausman tests found no evidence for this. If they are biased, they are biased downwards, and the true elasticity is smaller (less negative). All of the point estimates are small, suggesting that the demand for pharmaceuticals is not greatly responsive to price for this population. None of the estimates are significantly different from zero, so there is indeed no evidence found of any response to price.

Figure 9 Main Estimates of the Price Elasticity of Demand for Pharmaceuticals Amongst High Income Older People in Australia with 95% Confidence Intervals



7. Conclusion

This is the first study of the price elasticity of demand for pharmaceuticals amongst older people that draws on a natural experiment with a control group. Previous natural experiment studies have assumed the continuation of a trend, thus ignoring any changes that are contemporaneous with the policy change. I find no evidence that the elasticity is significantly different from zero for high-income older people. Since there is no evidence of endogenous CSHC take-up, the 'headline' point estimate is –0.1, which is consistent across the non-IV models. This supports the hypothesis that the elasticity is smaller than for the general population. It conforms with previous estimates for older people (McManus *et al.*, 1996; Contoyannis *et al.*, 2005). It does not conform to the surprisingly large estimates found for high-income older people by Contoyannis et al. Whilst the confidence intervals from almost all specifications include both negative and positive elasticities, economic theory rules out the latter. Given this, the range of plausible estimates is approximately -0.3 to 0.

These results have a number of implications. They suggest that the policy change is efficient in the sense that it has induced little excess consumption. As such its value to the recipient may be close to the cost of the program to government. On the other hand, the low elasticity implies that high-income older people do not need this additional benefit in order to purchase sufficient quantities of pharmaceuticals. However, the CHSC policy change was not motivated by the goal of ensuring adequate access to drugs. Its official purpose was to encourage saving for retirement.

The results have corresponding implications for policies aimed at controlling the cost of pharmaceutical insurance. For this population at least, increases to co-payments are unlikely to greatly reduce demand for drugs. The resulting saving to government may be little more than the increase in out-of-pocket expenditure. On the other hand, such increases to copayments would be unlikely to render pharmaceuticals unaffordable for the majority of high-income people. These results should not be generalised, however, to the low-income population of older people, who are likely to be more responsive to copayment increases.

Appendix: Supplementary Results

Table 6 contains regression results for the linear IV model. In the first stage results, the instruments are highly significant (p<0.000 in the F-tests). Whilst the main instrument (intervention) is not individually significant in the first stage, this is because of the large number of interactions included in the model. When the interactions of intervention are excluded from the model, its t-statistic increases to over 6.

Table 6 Estimated Coefficients From Linear IV Regression Model (Both Comparison Groups; All Years) With and Without Controls

	No controls			Full controls		
Variable	Estimated coefficient	Robust Standard error		Estimated coefficient	Robust Standard error	
group 1	-0.3288	0.1364	**	-0.0833	0.0826	
group 2	-1.1448	0.3844	***	-0.1843	0.2258	
Year 2001	0.0469	0.0440		-0.0673	0.0311	**
Year 2004-05	-0.1516	0.0406	***	-0.3327	0.0301	***
health concession card	-0.4568	0.4290		-0.1671	0.2746	
age				-0.0255	0.0259	
age squared				0.0003	0.0002	*
female				-0.0696	0.0227	***
personal income				0.0201	0.0167	
personal income squared				-0.0015	0.0012	
married				0.0270	0.0232	
high blood pressure				1.1369	0.0259	***
high cholesterol				0.8305	0.0353	***
diabetes				0.8981	0.0498	***
asthma				1.1082	0.0532	***
SAH - very good				0.0879	0.0226	***
SAH - good				0.1827	0.0252	***
SAH - fair				0.4458	0.0334	***
SAH - poor				0.6823	0.0541	***
constant	1.6862	0.4001	***	0.5583	0.9279	
p-value for Hausman endogeneity test		0.05			0.23	
1st stage results						
Intervention	0.2753	0.0559	***	-8.0869	10.2535	
group 1	-0.5093	0.0432	***	-0.4691	0.0438	***
group 2	-0.8908	0.0076	***	-0.7976	0.0255	***
Year 2001	0.0316	0.0063	***	0.0333	0.0063	***
Year 2004-05	0.0347	0.0059	***	0.0381	0.0059	***

age				0.0040	0.0093	
age squared				0.0000	0.0001	
female				-0.0087	0.0048	*
personal income				-0.0127	0.0054	**
personal income squared				0.0003	0.0005	
married				-0.0144	0.0056	***
high blood pressure				0.0170	0.0049	***
high cholesterol				0.0025	0.0061	
diabetes				0.0063	0.0060	
asthma				0.0128	0.0084	
SAH - very good				0.0004	0.0096	
SAH - good				0.0184	0.0095	*
SAH - fair				0.0212	0.0099	**
SAH - poor				0.0435	0.0119	***
Intervention*agecont				0.2186	0.2831	
Intervention*agecont2				-0.0014	0.0019	
Intervention*sex				0.0479	0.0515	
Intervention*ms				-0.0228	0.0512	
Intervention*hchbp				0.0011	0.0496	
Intervention*hcchol				0.0525	0.0542	
Intervention*diabcond				0.0715	0.0689	
Intervention*asthcond				-0.0211	0.0831	
Intervention*sah2				0.0497	0.0746	
Intervention*sah3				0.0505	0.0769	
Intervention*sah4				0.0771	0.0784	
Intervention*sah5				-0.0370	0.1226	
Intervention*y				-0.0026	0.0339	
Intervention*y2				0.0001	0.0025	
Intervention*yr2005	0.0503	0.0471		0.0614	0.0469	
constant	0.9416	0.0048	***	0.7248	0.3361	**
p-value for F-stat for instruments in 1st stage		0.00			0.00	

* Statistically significant at the 10% level
** Statistically significant at the 5% level
*** Statistically significant at the 1% level

Table 7 contains regression results for the linear non-IV model. The same variables are significant in the linear model as in the Negative Binomial model.

Table 7 Estimated Coefficients From Linear non-IV Regression Model (Both Comparison Groups; All Years)

	Estimated coefficient	Robust Standard error
Year 2001	-0.2150	0.1068**
Year 2004-05	-0.3996	0.1014***
health concession card	0.1278	0.0852
age	0.0964	0.5141
age squared	-0.0005	0.0035
female	-0.0742	0.0756
personal income	-0.0064	0.0504
personal income squared	0.0008	0.0035
married	-0.0133	0.0781
high blood pressure	1.1127	0.0886***
high cholesterol	0.7817	0.1195***
diabetes	0.9696	0.1786***
asthma	1.1726	0.1839***
SAH - very good	0.0388	0.0745
SAH - good	0.0805	0.0818
SAH - fair	0.3496	0.1296***
SAH - poor	0.7686	0.2469***
constant	-4.139	18.597

* Statistically significant at the 10% level
** Statistically significant at the 5% level
*** Statistically significant at the 1% level

The following five figures present the results of sensitivity tests for the elasticity estimates based on the regression and PSM approaches.

Figure 10 shows the results for a set of GMM count data models. As shown in the body of the paper, the preferred model results in an estimated elasticity of +0.11 (95% CI: -0.48, +0.69). With no controls, the estimate is slightly higher at +0.32. When survey weights are not applied, the estimate becomes -0.13. When additive errors are assumed (see Mullahy, 1997; Windmeijer & Santos Silva, 1997) the estimate is -0.05. Estimates are also shown for two-year and one comparison group specifications, which range from -0.14 to -0.02. The result is not sensitive to the exclusion of the (possibly endogenous) self-assessed health variable (2nd from right), or to the alternative cut-off in the proxy income threshold for couples in 2001 (far right). Thus with the exception of the model without controls, the

sensitivity tests shows that the point estimate does not vary greatly between specifications, ranging from -0.14 to +0.11.

Figure 10 Estimated Elasticities and Confidence Intervals, Sensitivity Analysis for GMM count data regressions

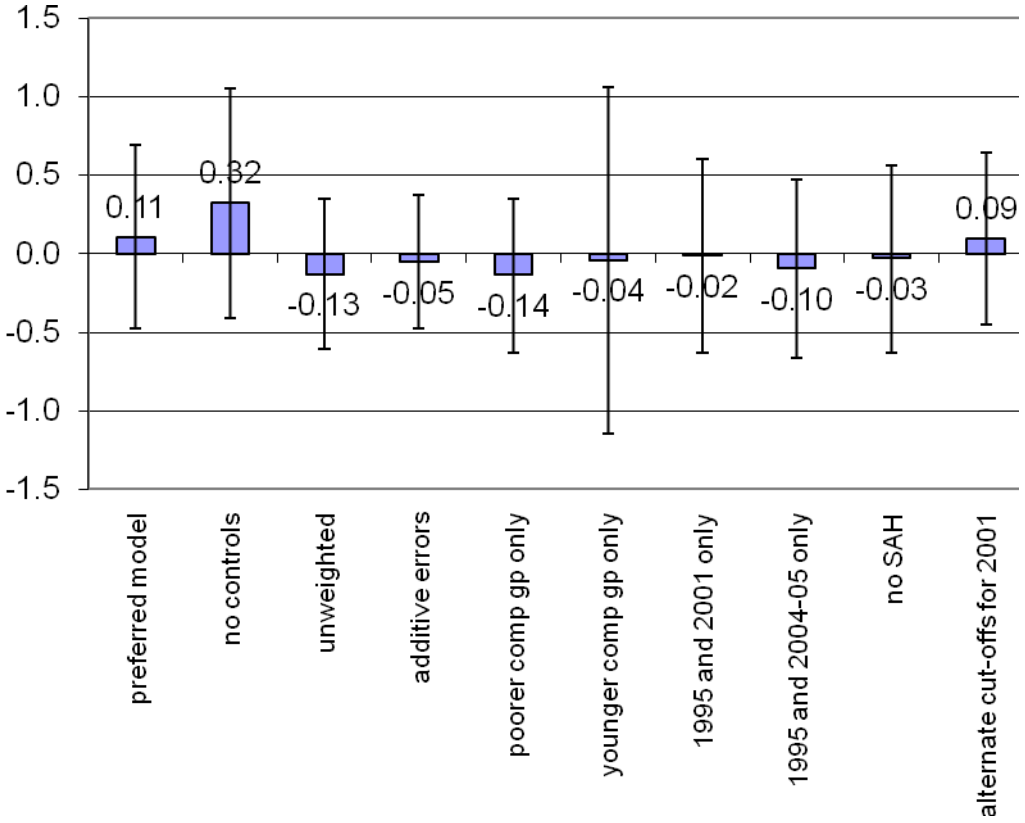


Figure 11 shows the results of sensitivity tests for the linear IV model. As shown in the body of the paper, the estimated elasticity from the main model is +0.14 (95% CI: -0.35, +0.64). The results vary slightly more here than they do for GMM. However, none of the estimates are significantly different from zero and none of the point estimates are less than -0.12.

Figure 11 Estimated Elasticities and Confidence Intervals, Sensitivity Analysis for linear IV regressions

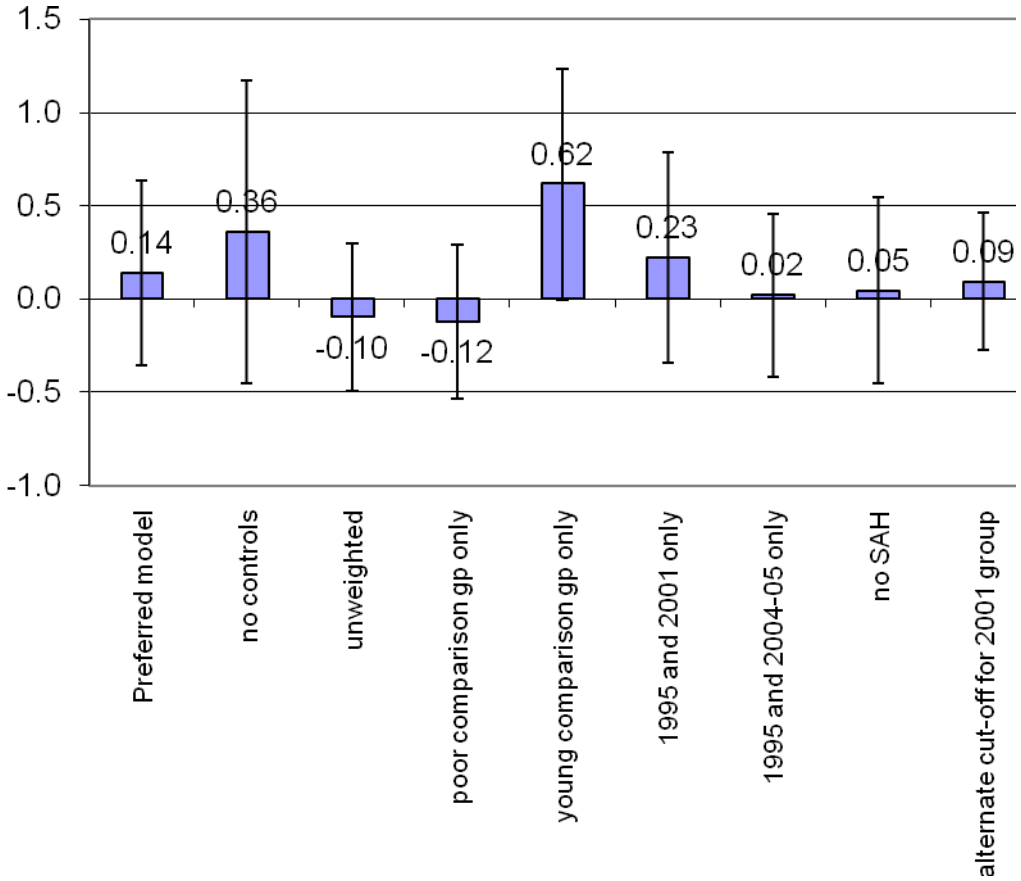


Figure 12 shows the results of sensitivity tests for the non-IV negative binomial regressions. The estimated elasticity from the main model is -0.11 (95% CI: -0.28, +0.05). The second estimate corresponds to a model without control variables. This estimate is larger: –0.35. A comparison of these two estimates suggests that much of the difference in consumption between card-holders and non-card holders reflects discrepancies in observable characteristics. The results of the unweighted model are similar to the weighted model, but the estimate is significantly different from zero due to a smaller standard error. The following three estimates are for individual years (1995; 2001; 2004-05). These vary from –0.32 to +0.02, well within the possible realms of sampling error. Recall that the sample for the non-IV estimates is restricted to the population whose income and age suggest they may have been affected by the CSHC reform. This corresponds to between 224 and 298 observations for each year. The difference in estimates for 2004-05 and 2001 is not

statistically significant (p>0.13). The result is not sensitive to the exclusion of self-assessed health (far right).

Figure 12 Estimated Elasticities and Confidence Intervals, Sensitivity Analysis for Negative Binomial non-IV regressions

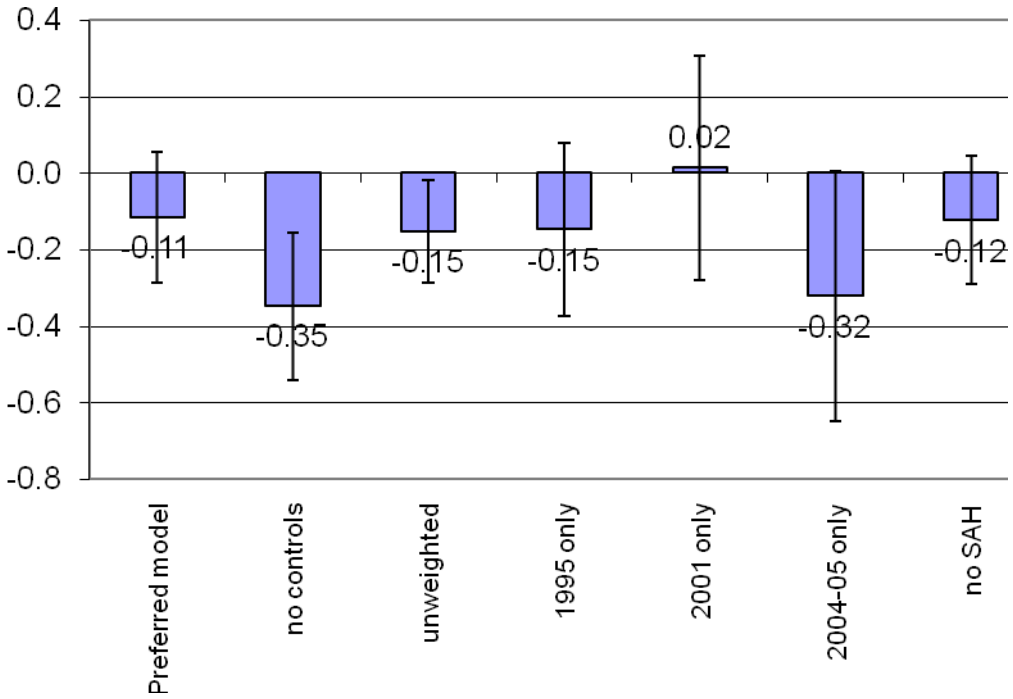


Figure 13 shows the results of sensitivity tests for the non-IV linear regressions. As in the corresponding negative binomial regressions, the contrast between the models with and without controls suggest that much of the difference in consumption is explained by differences in observable characteristics. When limited to individual years, the linear estimates do not differ from each other as much as the negative binomial estimates.

Figure 13 Estimated Elasticities and Confidence Intervals, Sensitivity Analysis for linear non-IV regressions

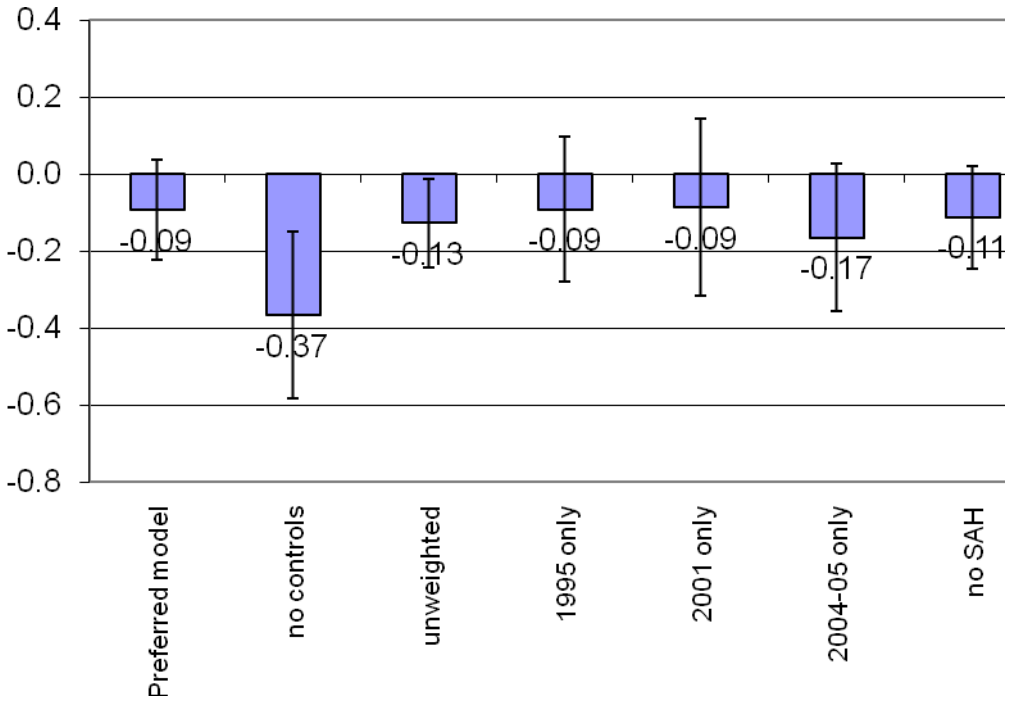
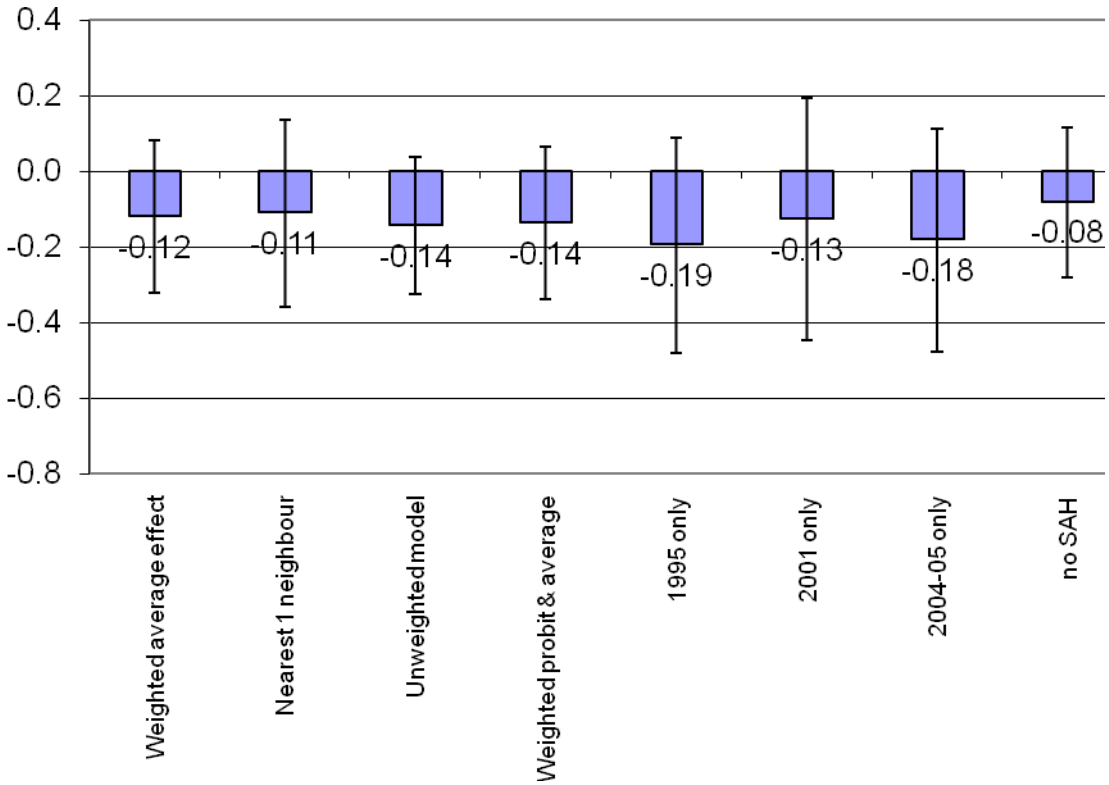


Figure 14 shows the results of sensitivity tests for the PSM results. The preferred approach yields an estimated elasticity of –0.12 (95% CI: -0.32, +0.08). The next estimate is the corresponding estimate using the nearest single neighbour for the matched comparison, which is very similar (-0.11). Unweighted PSM results in a similar elasticity (-0.14). The following estimate uses sample weights in the probit model for the propensity score as well as in the calculation of the average effect. The resulting elasticity is -0.14. When each year is considered in isolation, the estimate varies from –0.13 to –0.19. Each of these estimates is marginally larger than the pooled estimate, and well within its confidence interval. The PSM methodology does not guarantee that the results for sub-sets of the sample will be symmetrically distributed around the main estimate. The results are not greatly sensitive to the exclusion of self-assessed health (-0.08).

Figure 14 Estimated Elasticities and Confidence Intervals, Sensitivity Analysis for PSM



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